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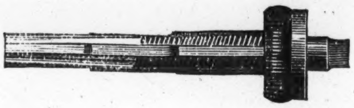

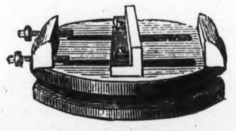

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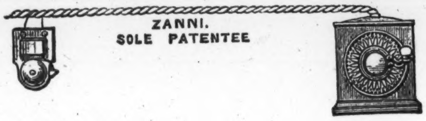

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THE TELEGRAPHIC JOURNAL AND

Electrical Review.

Vol. X.—No. 219.

ELECTRIC LIGHTING BY INCANDESCENCE.

I.

Now that the subject of electric lighting by incandescence is attracting so much of the attention, not only of scientists and inventors, but also of the general public, it may not be out of place to consider this subject from the beginning in such a way as to render it intelligible to those of our readers who although interested in this very fascinating subject, yet have not considered the law on which the system is based. At the same time we would express our views on the matter to those who are now employed in perfecting the various systems that have recently been brought before the public notice.

We will first of all consider the laws of the thermal phenomena of the electric current. When an electric current is generated in a voltaic battery by completing a circuit between its poles, heat is generated in the entire circuit consisting of the electro-motor and the wires connecting the poles. The circuit being closed a definite amount of heat is established throughout, but if the resistance of any particular part of the circuit is greater than that of the rest, then the amount of heat developed in that particular spot will be correspondingly greater, the heating effects in fact being proportional to the resistance. It will be seen, therefore, that the relative powers of metals for acquiring heat are directly as their retarding powers to conduction, or in other words, as their resistance to the passage of the current. For instance, if we take a length of platinum wire of a definite sectional area, and we pass sufficient current through it to heat the wire, say, to redness, then if we pass the same current strength through a copper or silver wire of the same sectional area as the platinum one, the amount of heat generated will be so comparatively small that no luminous effect will be produced, as the relative, or *specific*, resistances of silver or copper are very much less than the specific resistance of platinum, *i.e.*, the resistance of, say, 1 foot of platinum wire $\frac{1}{1000}$th of an inch in diameter would be about $5\frac{1}{2}$ times as great as the resistance of a foot of copper or silver wire also $\frac{1}{1000}$th of an inch in diameter, that is at any particular temperature. The well-known experiment of passing a current of electricty, of suitable strength, through a chain composed of alternate links of platinum and silver wire of the same diameter, illustrates what we have already stated, for whilst that part of the chain made of silver remains comparatively cool, the platinum becomes incandescent, although the current strength flowing through each link is the same.

It must not be imagined, however, that the temperature to which the platinum is raised is $5\frac{1}{2}$ times that of the silver or copper, although the temperature varies directly as the resistance; one reason of this is that the *specific heat* of platinum is less than that of silver or copper. It may perhaps be as well to explain what the "specific heat" of a substance means. If we take similar weights of various kinds of matter, and we heat them all with a definite quantity of heat, we shall find that the temperatures to which the various substances have been raised will be very different. Thus, for example, suppose we take equal weights of silver and platinum, and we heat each by the combustion of 1 lb. of coal, then we should find that the platinum has become heated to a much greater extent than the silver, the proportion being as 57 to 32·4; the proportion between copper and platinum is still greater, the relative values being as 95 to 32·4; in other words, if we had a certain weight of platinum, and we heated it by the combustion of a sufficient quantity of coal to bring it to a white heat, then, if we heated a similar weight of copper by the same amount of coal, the copper would only be raised to a dull red heat. The following show the various specific heats of the materials which have generally to be dealt with in connection with incandescent lighting:—

Copper	95
Platinum	32·4
Iridium	32·6
Carbon	20

The figures given are the relative specific heats of equal *weights* of the different substances, but, inasmuch as in dealing with the question of the resistances of the various substances, it is *bulk*, and not *weight* we shall have to take into account, that is, as we shall have to compare the relative resistances of wires of a definite length and *diameter*, and not of a definite length and *weight*, so we must have the relative specific heats of the materials, with reference to equal *bulks* or *volumes*; these values will be as follows:—

Copper	95
Platinum	79
Iridium...	61
Carbon	4·8

In other words, if we take a wire of copper of a certain length and diameter, and a wire of carbon of the same length and diameter, then a given amount of heat will heat the carbon

$$\frac{95}{4\cdot8} = 20 \text{ times (approximately)}$$

as much as the copper, and about 16 times as much as the platinum; the temperature, in fact, will vary *inversely* as the specific heat.

We have seen that the heating power of a current in a wire varies as the conducting power of the latter, or, as it is better to put it, the heating power varies directly as the *specific resistance* of the wire. Now, if we make a comparison of the specific resistances of the substances whose specific heats we have given, we find the following values:—

Copper	1·0
Platinum	5·6
Iridium	—
Carbon	6500

Since, then, the heating of a wire by a current depends directly upon its specific resistance, and inversely upon its specific heat the relative values of the various materials, as regards the heating they would undergo when traversed by the same current, would be as follows—

Copper	$\frac{1}{95} \times 100 =$		1·05
Platinum	$\frac{5\cdot6}{79} \times 100 =$		7·09
Iridium	———	=	———
Carbon	$\frac{6500}{4\cdot8} \times 100 =$		136,000·00

provided no change takes place in the resistance when the heating occurs; but metals *increase* their resistance considerably by change of temperature, whilst carbon *diminishes* its resistance.

The increase in resistance of metals by rise of temperature can be illustrated by connecting a battery to a length of platinum wire sufficient to raise the wire to a red heat, and then to further heat a portion of the length by a spirit lamp or other source of heat; this causes the resistance of the portion to become greater, and consequently less current flows and the red glow of the other portion of the wire diminishes. This experiment shows that in estimating the temperatures to which wires of different kinds will be heated by a current, it is their resistances at the ultimate temperatures which must be considered; thus, for example, the specific resistances of copper and platinum at the *same* temperature are as about 1 to $5\frac{1}{2}$, but we cannot therefore assume that the degree to which they will become heated by the same current will be as 1 to $5\frac{1}{2}$, since the platinum being heated more than the copper by the current, the relative resistances of the metals at the final temperatures which they will assume will be in a much greater proportion than 1 to $5\frac{1}{2}$.

In fact if we have two similar wires of different metals both traversed by the same current the relative temperatures of the two will be directly proportional to the resistances of the wires at those temperatures, and moreover the change in the resistances of the wires from their normal value will have a certain proportion to the degrees to which they have become heated. If a metal wire increases its resistance, say, 10 per cent. by an increase of 5° of temperature, then a further increase of 5° will increase the resistance 10 per cent. more, calculated on the new resistance. Expressed mathematically the law is as follows :—

$$R = rk^{\frac{t}{t'}}$$

Where r is the resistance of a wire at any temperature, k the amount by which this resistance would be increased by an increase of temperature t', and t the number of degrees of increase of temperature which would raise the resistance to R. Thus, for example, platinum doubles its resistance by an increase in temperature of 455° C., then we have for platinum—

$$R = r \times 2^{\frac{t}{455}}$$

For copper we should have—

$$R_1 = r_1 \times 2^{\frac{t_1}{255}}$$

Carbon, as has been stated, unlike metals, diminishes in resistance by increase of temperature. The rate of this decrease appears to be about 50 per cent. for a temperature of 2,500° C., consequently we should have for carbon—

$$R_2 = \frac{r_2}{2^{\frac{t_2}{2500}}}$$

(*To be continued.*)

REVIEW.

Report of the Lightning Rod Conference. London : E. & F. N. SPON, 16, Charing Cross.

THE impression which this report gives us is, that a number of able scientific men have worked hard to obtain every possible information on the subject with which they had to deal, and that after the lapse of three years they have made the best they could of a bad job. Letters have been written right and left by the conference to manufacturers and others who had it in their power to furnish information on the subject, and the result has been ridiculously small ; for instance, out of *sixty-five* manufacturers who were written to, only *eight* deigned to give replies, and the differences of opinion which exist in the latter must only have embarrassed the delegates, if indeed these gentlemen allowed their opinions to be guided by the information given, which seems doubtful.

Each of the eight firms referred to have stated what descriptions and arrangement of conductors they have erected ; and although in several respects, as we have stated, these differ, yet the buildings on which each firm has fixed their protectors have in no cases suffered from lightning, thus showing that all the systems are efficient, though at the same time they may all have been unnecessarily complicated and expensive. The real question at issue, viz., "what is the minimum protection that is necessary for any particular building," is very little helped by the replies received.

An analysis of the answers shows that two firms recommend the use of insulators ; one firm objects to soldered joints, whilst three only consider them necessary. With regard to the area which the rods will protect, the replies are ambiguous. One firm states that "the area protected may be taken as equal to from three to five or six times the height of the conductor" ! Another says "the area protected is *supposed* to be a radius equal to the height of conductor." A third, "thirty to forty yards" ! As is pointed out in appendix B, except in the use of copper and the necessity for reaching damp earth, no two manufacturers agree. One important point may be gathered from the replies, and that is that no smaller conductor than three-eighths of an inch in diameter should be used ; as to the maximum size opinions vary.

A large number of instances, in many cases illustrated, are given of cases where buildings have been struck which have *not* been furnished with lightning protectors ; what good such information is it is difficult to see. There are very few, if any, persons who doubt the utility of protectors when properly erected ; what it is required to know is "what arrangement of protectors are necessary to protect a building," not "are protectors necessary." It certainly seems as if the conference have piled in their report all the information which they have received, without considering whether there was any utility in it or not. Thirty-six pages, for instance, are devoted to a catalogue of the literature which has been published on the subject of lightning conductors. As a padding, no doubt, this list is useful, but it hardly serves any better purpose.

The report itself occupies but twenty pages out of a total of 260, and although we do not complain of its brevity, for we think the conference has done wisely in explaining their views as concisely as possible, yet in several respects it is imperfect. For instance, with regard to testing, no suggestions are offered as to how it ought to be done. The report contents itself with stating that testing is necessary, and that several methods have been suggested for making the tests.

It is stated that "the efficiency both of the conductor and of its earth terminal should be annually tested," we quite agree ; but with regard to the earth terminal test, we should have been glad if the conference had stated how this test is to be taken. We know of no method. It has certainly been stated more than once that the test may be made by connecting one wire to the earth terminal and another to a gas or water pipe, and then seeing whether there is any resistance in the circuit. Such a test is no test at all, it simply shows whether the mass of earth between the water or gas pipe and the earth terminal has any resistance ; if the latter is found to be zero, it proves nothing, for, although it shows that the lightning has a free path from the conductor to the water or gas pipe, it does not show whether the latter pipe is capable of carrying off the discharge effectually. We know of at least one case in which an earth was tested by the method we have referred to, and was concluded to be good, when in reality it was defective.

In the "Code of Rules for the Erection of Lightning Conductors," with reference to terminal "points" it is stated that "it is desirable that these points should be so platinised, gilded, or nickel plated as to resist oxidation ;" this hardly accords with a rule with reference to "ornamental ironwork," as it is there stated that "it is not absolutely necessary to use any other point than that afforded by such ornamental ironwork, provided the connection be perfect and the mass of ironwork considerable." If the points on the special terminals require to be gilded we should imagine it was equally necessary for the points on the ironwork to be similarly treated, but there is nothing to lead one to suppose that the latter course is recommended by the conference.

With regard to "protection" it is recommended that "copper rods to the height of ten feet above the ground should be protected from injury and theft by being inclosed in an *iron* pipe reaching some distance into the ground." The result of such an arrangement would be, we imagine, to form a complete galvanic cell, the iron pipe forming one electrode and the copper rod the other, whilst the water which would certainly collect in the pipe, by being conducted down the copper rod, would complete the arrangement ; under these conditions the iron pipe would stand a poor chance of durability, whilst the copper rod, especially if formed of stranded wires, would not be improved by the action which would take place.

(*To be continued.*)

THE TELEPHONE CASE.

In the High Court of Session. First Division, Edinburgh.

Before LORD M'LAREN.

THE UNITED TELEPHONE COMPANY, LIMITED,
v.
ALEXANDER MACLEAN, EDINBURGH.

(*Continued from page* 70.)

At an angle of something more than five degrees, and less than twenty degrees ?—Yes, at an angle ranging from five to twenty degrees.

To your mind, as a scientific man, you had the impression that it should be loose and free to move ?—I knew of the law of vibrations, and that it would be more ready to respond to the sounds if loose.

Was there anything to suggest to your mind that the implement would be got to speak if you fastened that tongue down ?—No.

In the state of your knowledge at the time, and from what you had heard, was it your impression that it ought to be free at one point ?—Yes.

Did you think that the attachment round its periphery should be avoided ?—Yes, as it would give a chattering effect.

There was nothing to convey the notion to you that a workman of ordinary skill could make such an instrument ?—Certainly not.

Did you drop your experiments with the instrument in Glasgow as a failure ?—Yes, I was confident it was in good hands.

Did you consider that you had got the idea of a completed instrument?—I felt convinced that I could not make the instrument to show it to my class. I could have got such an instrument from Bell, but I preferred to leave it in his hands.

You are aware of the progress which Bell afterwards made?—Yes.

You have read the specification of Mr. Morgan-Brown (Graham Bell's patent)?—Yes.

In the specification do you find any improvement on the instrument shown you at Philadelphia?—Yes. One was instead of putting the battery in the line wire through the electro-magnet to use a permanent steel magnet, no doubt as an equivalent of the electro-magnet; an ordinary electro-magnet might have been used, but an ordinary steel magnet was found sufficient.

Have you an equivalent for the permanent magnet in the soft iron core with the local battery?—Yes.

Is the same effect produced by a permanent magnet and an electro-magnet its equivalent, and do you find this in Bell's apparatus as shown in the specification?—Yes.

What are the advantages obtained by these improvements?

Sir William here described a telephonic circuit, pointing out that under the varying atmospheric conditions the resistance of the line varied, and while at station A electro-magnets would be more powerfully affected by the increase of current flowing to line, the electro-magnet at the receiving end would be less affected in consequence of loss of current through leakage. The advantage of an electro-magnetic arrangement with a local battery was that the battery was not in the line wire. The second is the improved form of transmitter in which the goldbeaters' skin, replaced by the steel diaphragm, is used.

What is the advantage of the steel diaphragm?—The great material improvement is its non-liability to have its adjustment disturbed by the breath of the operator. This improvement alone with the faulty arrangement of the battery in the line wire would have been a substantial improvement.

Did the membraneous diaphragm suggest to you the notion of the metal one?—No. They are very different.

Lord M'Laren: That improvement did not suggest itself to your mind as an obvious development?—Certainly not, my lord; I was greatly surprised to see it.

Lord Advocate: Did the substitution of the metal involve merit and invention?—Certainly.

With all your knowledge of the properties of a magnet it did not occur to you a thin plate vibrating in a magnetic field was to set up an action capable of producing vocal sounds?—No.

In the transmitter alone there were two improvements involving merit and invention?—Yes.

They would not have been suggested to a workman of ordinary skill?—Certainly not.

What improvements did Bell afterwards make in his receiver?—Instead of the loose tongue, substituting an iron or steel plate, and attaching all round its ridge to a solid body, and the substitution of the permanent steel magnet.

Taking the case of the Bell telephone with the permanent magnet. In this case you have an identical instrument at each end, the one performing the duties of a transmitter at the one end, and the other the duties of a receiver at the other?—Yes, and they fulfilled the same function.

Was it, in your knowledge, known that the magnet could set up in the steel plate fastened all round such vibrations as to produce vocal sounds?—No.

Did it involve merit in invention?—Great merit in invention.

Did such an improvement never occur to you or anybody but Bell?—Yes, a great many people had the chance, as it appeared in *Engineering* on the 22nd of December, 1878; but we were before them, the Morgan-Brown patent being taken out on the 9th of December.

Was it the known property of a steel plate fastened at its circumference that it could receive from the voice such vibrations and receive from the magnet such vibrations at the other end?—Not known.

Then there were apparently three novel and meritorious improvements since you saw the invention at Philadelphia, and they made the apparatus for the first time a commercial success?—Yes.

You presided at the British Association meeting at Glasgow in in September, 1876?—Yes, over section A and delivered an address.

I suppose you told all you knew? You did not keep, at all events, anything back?—No; I may not have told everything, but I did not wilfully keep anything back. I showed the apparatus, and it was on the table for days. It was examined by the scientific men present, who expressed doubts, and a good deal of incredulity prevailed.

Lord M'Laren: Was it fitted up with a wire?—No, it was not connected up.

Lord Advocate: Did you think that anything said in your address would enable a workman of ordinary skill to make the Bell telephone?—Certainly not.

You had tried and failed to make it speak?—Yes, and I was very ambitious to show the association what it had done.

You are also acquainted with the Edison patent—the carbon transmitter?—Yes.

What in your opinion is novel and meritorious in it?—A practical method of transmitting sound.

Was there in the case of Bell's a certain weakness or comparative weakness in the transmitting power?—Yes, it was too feeble for long distances, in consequence of a want of sufficient power. An increase of power would be a great improvement, and this Edison accomplished.

In your judgment is the telephone with an Edison transmitter and a Bell receiver as good an implement as is known at present?—Yes.

Has it been of current and practical value?—Yes, of great value.

On being shown drawings of Edison's transmitter as patented in 1877, and as amended by disclaimer, Sir William proceeded to describe the transmitter as so made.

Lord Advocate: What function does the carbon fulfil in this transmitter?—It gives rise to diminished resistance by pressure, and to increase that resistance when the pressure of the cork is diminished.

Lord M'Laren: That is resistance to the transmission of the electric current?—Yes.

Does it possess the quality of, under slight degrees of pressure, decreasing the resistance to the passage of the electric current?—Yes.

Are there materials which possess the same qualities?—Yes.

Is there anything in so eminent a degree as carbon?—Nothing that I am aware of.

What is carbon—is lamp-black carbon?—Yes; the lining of gas retorts and plumbago are familiar forms of carbon.

Are these all within the specification?—Yes.

In this form does it possess the property you described?—Yes, certainly. (Shown an Edison transmitter.) There are so many different varieties that I am not prepared to say that this is one of the forms in general use.

Is the notion for the making of this transmitter clearly conveyed in the specification of 1877?—Yes, essentially as in the specification.

So that a workman of ordinary skill would be enabled to construct the instrument?—Yes. (Witness shown a receiver used by respondent.)

Sir William: This is most distinctly Bell's patented receiver, and is distinctly described in Morgan-Brown's specification. Everything that is in Bell's is here, and nothing more than is in Bell's, and all perform the same operations. (Witness next shown Theiler's transmitter.)

Sir William: This is most distinctly Edison's carbon transmitter. (Here witness gave a technical description of the action of this transmitter.)

Sir William detailed at considerable length the action and effect of the suspended carbon pencil and Theiler's transmitter, and described the action of the transmitter in and out of use.

Lord Advocate: Is the operation here different from that where we have one piece of carbon more or less squeezed?—No. They are there for the same purpose and produce the same result; there may be molecular pressure.

Have you molecule against molecule?—Yes.

And precisely the same when working with one piece of carbon?—Yes, they give good results by a difference of pressure.

Is there anything of the jolting action?—There might be something of the jolting action, but it would impair the action by making and breaking the circuit.

You have told us that it is essential to have a continuous variation of pressure and a continuous current?—Yes.

Would the jolting motion be a make and break?—Yes; and the articulate sounds would be impaired and the effect would be as of one speaking through a cold.

It is, therefore, your opinion that there can be no virtue in a speaking telephone having a jolting motion?—No; it is a difficulty.

Upon full consideration, do you adhere to the statement that the Theiler transmitter is simply Edison's?—Yes.

It is essential or material to the specific action here in question in what precise form the carbon particles should be presented to each other?—No.

Speaking of Edison's tension regulators, Sir William said that all the materials specified would produce the result claimed.

Does carbon possess the properties which are here in question?—Yes, and is used in this way in connection with the Brush electric light.

You have read the provisional specification, and in your mind the phonograph was claimed in the provisional specification?—Yes. It is a thoroughly accurate account of the phonograph or instrument for recording and reproducing sounds.

It would be useless if it did not reproduce the sounds?—Yes.

The virtue in the phonograph depends on the reproducing what is recorded?—Yes.

Lord M'Laren: Do you think that the difference between the pendulum motion in the Theiler and the screw action in the Edison likely to affect the molecular action?—Not at all.

Lord M'Laren: Is there any difference in elasticity of the different forms of carbon you have spoken of which might affect the operation?—Yes, there is a great difference in their elasticity. The kind of carbon best adapted for the transmitter is soft carbon. The articulation becomes impaired by the jolting action when the sound is too loud. For some purposes hard carbon is to be preferred, for others soft.

Lord M'Laren: Mr. Edison here contemplates the connection between carbon and cork, whereas in Theiler's we have connection between carbon and carbon.

Witness: There must be connection between carbon and a conductor.

Lord M'Laren: Would that in your opinion make a difference in the efficiency of the instruments?—Not at all.

Lord M'Laren: On the whole your opinion is that the instrument you have seen is a mechanical variation of the process described in Edison's specification?—Yes.

Cross-examined by Mr. Mackintosh.

I suppose it would be correct to say that the pencils in Theiler's are in loose contact?—Not quite so; they are always in firm contact.

The contact is loose in proportion to the weight upon the carbon pencil?—Yes.

According to your view there is no special virtue in looseness of the contact?—There is a virtue and a vice. Assuming that there is no make or break it is simply a matter of adjustment.

It does not appear to you that the instrument has been designed to produce looseness with break and continuity?—No.

It would not occur to you that looseness of contact had been aimed at?—There is no looseness of contact. It is designed to prevent looseness of contact. If there is looseness it is a defect, and should be avoided.

Is the importance of looseness of contact in such instruments a matter of controversy in the scientific world?—Not to my knowledge.

Nobody of eminence in the scientific world has maintained that it was a virtue?—I am not prepared to say what any one of eminence may have said, and I am not aware if any one has said so, that it has been thought worthy of consideration.

Delicacy of contact is a virtue?—It is undoubtedly a virtue.

And looseness of contact a vice?—Delicacy, in my opinion, is a virtue for some uses, and in others a vice.

Was Hughes' microphone not the first instrument with what you call delicacy of contact introduced?—No; I think not; we had it in Reiss's.

Was this not the first instrument where a breach of continuity was exhibited?—No; so far as I know.

Had any of the instruments made on Edison's plan given the delicacy of contact discovered in the microphone?—I consider it to be identical with the instrument described by Mr. Preece at Plymouth. I have never seen any of the instruments. Mr. Preece's descriptions of Edison's instrument were exactly the same, except in looseness of contact.

Was the looseness of contact in the microphone not published as a great discovery?—It was for a time.

Was it not maintained by Hughes and others that the virtue of the instrument consisted in the delicacy of adjustment? Were not results produced after the introduction of the microphone which had never been produced before? A degree of loudness?—Yes.

Was the difference in degree of loudness the only thing?—In so far as the articulation was concerned the articulation was less distinct; the use of the microphone was to magnify sound, so that a fly walking might resemble the tramp of an elephant.

Are you aware that Hughes, in 1878, suggested the carbon pencils?—Yes.

Theiler's transmitter is a modification of Edison's?—Yes, as Hughes' microphone was. The great merit in Hughes' microphone was that you could get it for 1½d., and this was the merit claimed for it by Preece. The looseness of contact which was a great virtue of Hughes' microphone is obviated by the ingenious suspension in Theiler's instrument. (Witness shown a Crossley microphone.)

How is that in respect to looseness of contact?—It is firmer than in those of Hughes.

You heard the lecture on Hughes' microphone read before the Society of Telegraph Engineers?—Yes.

Did you protest against the novelty of the invention?—I do not think that I spoke at the meeting at all.

Were you entitled to speak?—Really I do not remember. The effects were beautiful, but I had never the slightest doubt but that they were the same as Edison's. The great lightness of the contact being the important matter for the curious application which Hughes gave to it.

Reference having been made to the controversy between Professor Hughes and Mr. Edison, Sir William said he was happy to say that Mr. Preece is now on the best terms with Mr. Edison.

Before Hughes published his instrument had there ever been, to your knowledge, a transmitter made and commonly used in combination with any of the figures in Edison's patent of 1877?—I do not know.

Have you ever seen an instrument made and used with any of those figures?—No.

Was there any transmitter in use in this country prior to March, 1878; was the Bell transmitter?—I do not know of any having been in common use.

Have transmitters other than Bell's been disused? You do not consider that Bell's is so good a transmitter as Edison's?—Yes, I consider it as good—the fault is comparative loudness; and, on the other hand, it has the advantage of no local battery.

Your theory is that the Theiler transmitter works by simple variation of pressure, and you cannot conceive any other law in its action?—I have not tried to conceive any other law.

In the make and break system has there ever been any other instrument made except Reiss's?—Yes, but they have all been designed to transmit a musical note.

Are you prepared to say that it is impossible to have a make and break motion consistent with the transmission of articulate sounds?—It is essential.

LORD M'LAREN: So that by a make and break system alone articulate sounds cannot be produced?—They cannot.

LORD M'LAREN: Has this ever been put to the test?—I am not aware of any experiment.

EXAMINATION RESUMED.

How does carbon compare with plumbago in point of elasticity?—No experiments made would allow that question to be answered.

Is gas carbon more or less elastic than plumbago?—It is more elastic than plumbago.

LORD M'LAREN: I thought it was the action of the minute particles that was the active principle in the telephone?—Yes.

In Edison's specification what form of carbon does he describe? Does he describe any form of carbon but plumbago?—Lamp-black and plumbago are the only two described.

Is there any form of carbon used in a solid form besides plumbago?—I do not know.

Is there any preference given to plumbago over the other materials for the tension regulators?—I believe so.

Have you ever experimented to find if the resistance varies under pressure?—I have not.

If it were the fact that gas carbon does not vary its resistance under slight changes of pressure would that affect in your opinion the identity of Edison's instrument with Hughes'?—Not at all.

If Theiler's instrument did not act by pressure would it not be of a different principle from Edison's?—It would show that the demonstration could not be given.

In the Theiler instrument you have what you call a diaphragm, and the diaphragm lies loose?—Not altogether loose.

The diaphragm is cemented to the cork?—Yes.

The cork is loosely attached round its periphery by the lid of the instrument?—Yes. In Edison's instrument the cork is fastened to the diaphragm, and the cork, in so far as it is placed, and in working is free to move with the diaphragm; the cork is therefore free to move.

In Theiler's instrument when the diaphragm vibrates it cannot move the cork forward bodily in the same way as in Edison's?—Yes; the cork is a highly flexible substance. The cork in Theiler's is put forward in virtue of its elasticity, the tympanum being a compound of iron and cork.

The pencils are moved by the tremor given to the cork by the tympan. Does not the movement reach the pencils by means of a tremor passed through the cork?—It reaches them by vibratory movement which may be called tremor by the intense variation of air pressure on the tympanum.

Had the membrane transmitter its disc fixed all round?—Yes.

Did it not occur to you to fasten down the metal disc?—No. My impresson was that by attaching it too hard all sound was killed. The membrane in the goldbeater telephone is fixed by stretching on this by pressure.

Would a current passed through a Bell telephone keep the disc down?—Yes.

Did you explain that at the British Association?—No; the possibility of a current pressing down the disc was a very likely thing to be spoken of, but I had always found the action choked.

Shown the article in *Engineering*, said it was not sufficiently indicated to fix the disc down.

LORD ADVOCATE: Is looseness of contact not a rather loose expression in this controversy?—Yes; but in the action of the microphone it is correct.

LORD M'LAREN: What would be the correct expression?—Delicacy of contact; contact under pressure.

LORD ADVOCATE: The term looseness of contact implies make and break, whereas delicacy implies continued with slight continuity?—Yes.

Is there anything in Hughes' microphone which is not in Edison's?—Nothing.

Did Edison's specification disclose everything essential for the working of the microphone?—Yes, everything.

Would the elements which work advantages in Hughes' be detrimental to the good working of the articulating instrument?—I think they would.

Do you see any advantage in multiplying the carbons?—I do not see it, decidedly.

I suppose the telephone did not take the market at first; it takes the public time to believe in such things?—Yes. (A laugh.)

Could it be so refixed as to make the make and break arrangement?—Under certain arrangements it might be so made.

Suppose for a moment that the respondent's transmitter is useful for articulate speech, and that it has a make and break arrangement?—I would consider it a defect, although the instrument might still speak.

Do you find the idea of using compressed plumbago in the specification?—Yes, and an ordinary lead pencil would produce the same results.

All these instruments are essentially the same, they work in the same way and produce the same result.

LORD M'LAREN: Are you of opinion that under the make and break system it is only possible to produce sound notes audible, but not the more complex which enter into articulate speech?—Yes.

You are speaking from scientific investigation?—Yes, from the experiments of Helmholtz, Prof. Fleeming Jenkin and others. And you are further of opinion that these articulate sounds can only be made by continuous currents?—Yes.

(Court rose for lunch.)

On resuming, Mr. FALCONER KING, chemical analysist, was called. He testified that he had examined a piece of carbon taken from one of the instruments supplied by the respondent.

Mr. JOHN IMRIE, M.I.C.E., managing engineer, London, was next called.

LORD ADVOCATE: You have been in business as a managing engineer and patent agent?—Yes, about sixteen years.

And have devoted great attention to the progress of discovery in regard to the transmission of sound by electricity, which is comprehended in the telephone?—Yes.

And are acquainted with the Morgan-Brown patent, and you have studied the patent as amended by disclaimers?—Yes.

What, according to your notion, are the novel features of Bell's invention as now disclaimed?—A diaphragm or tympan put in motion by the variations of the electric currents in a line wire, and that they produce corresponding results at the end of the wire in a similar instrument; they were novel at the time the patent was taken out; it was a new idea.

You are aware of the instrument for the transmission of sound prior to 1878?—Yes; but they worked by make and break arrangements, and although capable of producing a musical note of a given pitch they were unsuited for the production of the ever-varying pitch and volume in articulate speech. In my opinion no apparatus which makes and breaks the circuit can transmit the open and close syllables which constitute articulate speech. I am aware that many efforts had been directed to the achievement of this end, but without success, and there was no method for this prior to the Morgan-Brown patent.

Was there anything to lead you to fix down the disc?—No; but something to lead me to a different notion.

In your judgment would an inspection of the instruments shown at Glasgow, coupled with the address of Sir William Thomson before the British Association there, have enabled you to construct a Bell telephone ?—Personally I do not think I could without fresh invention.

(Shown instrument used by respondent.)

It is a complete imitation of the instrument patented by Morgan-Brown, and is only different in shape.

You have studied the specification of Edison's patent of 1877 as amended by disclaimers ?—Yes ; and am aware that his claims have been reduced from thirty to three.

What is the claim as so amended ?—It is for such a combination as to produce variation in the electric circuit and reproducing these operations at the distant end. This is obtained by introducing into the circuit something which changes rapidly under pressure by partially conducting substances, chiefly carbonaceous substance.

Carbon has this property in a very remarkable degree ?—Yes, and the use of other forms of carbon, such as plumbago, would have suggested themselves ; even every substance has been found to answer in a greater or less degree.

Was the invention both novel and meritorious ?—Yes, novel and meritorious, ingenious and useful. (Reference being made to the fibre tension regulators, witness said he had never seen an instrument made as described in specification, but had no doubt it would work well.)

(Shown a Theiler's and Crossley's transmitter.)

Had no doubt they were both within the claims of Edison. In all you have the same materials producing the same results by the same means.

In the case of the Bell and Edison specification were they sufficient in your opinion to enable a workman of ordinary skill to make such instruments ?—Yes.

The phonograph is capable of being made a receiving and a reproducing instrument ?—Yes, and this is clearly disclosed in the specification. Theiler's instrument I believe to be a modification of Edison's.

Witness described at length the construction and working of the two instruments, and gave it as his opinion that they were only different in shape.

Mr. Imrie : It has been suggested that in the Theiler you would never have a pressing, but a jolting action, and it is not intended that it should act so, as is shown by the hanging of the weight arrangement. Would there be any advantage in having a jolting motion ?—No ; and the effect would be as if listening to a man with a stutter. The jolting motion would break the circuit and be a defect in the speaking telephone.

Cross-examined.

Is the diaphragm in the Edison invention an essential part, and one of the elements of the claim ?—Yes.

Do I understand you to mean by a diaphragm a flexible diaphragm? —Yes ; believe glass or wood would act instead of the metal, although have not tried it experimentally, and merely speak from general knowledge and from their action under other circumstances.

Edison must have a diaphragm or something equivalent ?—Yes ; and it may be a disc—a thin disc in a tense condition.

In your theory there are two factors? pressure and a partially conducting substance ?—Yes.

Would a Reiss's instrument work with one of the new forms of transmitter ?—Could not say.

In the membrane telephone, the Bell which was disclaimed owing to prior publication, you have the membrane fixed down, and the only difference between the membrane of the telephone and the Bell telephone was that in the former the disc was of skin, and the latter of metal. Would the one not have suggested the use of the other ?—No, it would have required inventive power to substitute the one for the other.

Lord Advocate : Do you find that Edison had in view not only proximity, but extent of surface ?—Yes, so that if there is any virtue in a larger surface he knew it.

Given the knowledge which Edison gave to the world it was easy to get at the microphone of Hughes ?—Yes, the suggestion was obvious.

Professor Fleeming Jenkin was now called. After the usual formalities of deposing and admission that he had devoted much time to the study of electrical phenomena, gave it in answer to the Lord Advocate as his opinion that the information disclosed in the Morgan-Brown patent was novel and thoroughly useful, and the invention disclosed was novel and thoroughly useful. In so far as it reproduced a fac-simile of the waves of air which the voice produced at the sending end, fac-simile is by far the best word to describe what is actually done.

Had anybody succeeded before Bell in reproducing the air waves? —No.

What had been done before Bell made his invention ?—We had the musical note produced, *i.e.*, the same number of waves per second, but the distinction between the musical notes and the articulation of the human voice is that the latter depends on the waves, and their particular shape is reproduced by Bell, and this is done by the aid of the telephone.

And by a closed circuit ?—Yes, and by the production of an electromagnetic force in that circuit. It is an essential part of the actions that the continuity of the circuit be maintained. The sounds vary according to a very complex law. A musical wave may be a continuous wave, but the sound which it reproduces may be very unlike the one transmitted. In the telephone we have the same produced, and this depends on the fac-simile of the wave itself being reproduced. (Shown telephone used by the respondent.) I have not the slightest doubt that it is precisely Graham Bell's patent.

Was there any close approximation to the telephone prior to the Morgan-Bell specification ?—Yes, the water telephone.

Was the invention of Bell new and meritorious ?—Yes, remarkably meritorious.

Have you examined the apparatus known as Theiler's ?—Yes, it is the same as Edison's specification of 1877.

You have seen and read the alleged prior publications ?—Yes, but I do not profess to have read every word of them. But nothing has been shown to me that would indicate anticipation of the inventions of Bell and Edison.

Cross-examined by Mr. Mackintosh.

In Edison's patent would it be correct to say that the resistance of the line is altered by varying the density of the electric tension regulator ?—Yes, but I do not know upon what it depends. It does depend, but I do not know how far it is limited to change at the surface of contact—some change of the density at the surface must occur.

Asked if the *modus operandi* described by Edison as to his tension regulator, witness protested against the name tension regulator, which he characterised as simply abominable, adding that he did not consider Edison an authority on the *modus operandi*, and that his language throughout in the letterpress was like his English, bad !

Asked if a particular form described in the specification had been made, witness said :—I have no knowledge on the subject, but all the instruments are essentially the same.

In your books on electricity and magnetism, in speaking of Edison and Hughes, you say Hughes discovered the microphone ?—I believe he did so.

Do you find the idea of the microphone in Edison's specification ? —Yes. Edison applies his invention to the mass of sound, Hughes to the microscopic.

Has Hughes' invention anything to do with the question before us ?—No.

Has lamp-black and plumbago all the properties for the varying of the current ?—Yes, they have all the necessary properties.

Lord M'Laren : Although you cannot explain in what respect the tension regulators act, you are quite satisfied that the pressure existing in the carbon is what favours the transmission of sound ?—Yes.

Lord M'Laren : It follows from that that the carbon-points are constantly in contact, and wherever there is not connection the action is defective ?—You might recognise the articulation, but it would be broken, and this would go on every time the circuit was broken.

Lord M'Laren : Is it correct to say that a semi-conducting material is the proper material for that purpose ?—It is quite proper.

Since Mr. Edison's patent has it been discovered that many other substances have the property to some extent ?—Yes.

Does gas carbon still continue to be one of the best and most suitable materials for that purpose ?—I do not attach much importance to one kind of carbon or another.

Is it, in your opinion, of any importance that the pressure should be produced by gravity or by the substances being held together by a spring ?—Of no importance, both produce the same results.

(*The Court then rose.*)

Thursday 26th.—On the Court resuming this morning, the complainers called Mr. Edward Johnson, sometime assistant to Mr. Edison, at Menlo Park, now general manager to the Edison Electric Light Company of Great Britain.

Lord Advocate : Were the transmitters described in Mr. Edison's specification practically used over a distance ?—Yes ; they were used as transmitters along a distance of two miles, and commercially used by the Western Union Telegraph Company of America, between their offices for private purposes ; and this was in the year 1877.

Did it work well ?—Yes.

And it had done so in the laboratory of Mr. Edison ?—Yes ; it there worked well.

Was the mixture of plumbago and silk fibre as described by Edison actually used ?—Yes.

Were the other modifications shown in the plans used in the laboratory ?—Yes, but not elsewhere, and my opinion is that they may be practically used, and the form of transmitters used at the present day are merely modifications.

Are the modifications of adjustment what you refer to ?—Yes, the main difficulty we had to contend with was to preserve continuity of the circuit, and the various modifications have been made to preserve the contact ; by the use of springs of various forms, weights, and screws, and by various other devices, all for the purposes of permitting the diaphragm having the necessary vibrations without breaking the circuit.

By a hanging weight, as in Theiler's instrument, this may be effected ?—Yes, and the idea is to keep a constant pressure.

(Shown a Theiler's transmitter.) The weight here produces the same results as the springs, &c., in Edison's specifications, and there is an indication that the functions of inertia are also to be used.

Would it be correct to say that it is a desideratum to have in each case an initial, but yielding, pressure ?—Yes.

Has the phonograph been used for the transmission of speech ?—Yes, have done it many times, the phonograph acting as the human voice. For transmitting messages over long distances it has been used as a relay or repeater.

Has the phonograph proved useful as a part of a telephone apparatus ?—Yes.

Cross-examined by Mr. Mackintosh.

For how long was the transmitter you first referred to and shown in figure 1 used ?—I cannot say ; certainly for some weeks.

Was it then discontinued ?—Cannot say.

Was there any other form of instrument introduced to supersede it ?—There were numerous modifications from time to time, and they are still being made.

Looking to the letterpress of the specification, does it show you what material is used ? Is it anywhere indicated that it is to be a form of carbon ?—Yes, by naming lamp-black and plumbago.

Do you find it anywhere indicated what the form of carbon is to be ?—No.

In the tension regulators of Mr. Edison there is nothing to show that any one of them may not be used?—No.

What material is now used?—Carbon.

In what form?—In almost every form it is possible to be put in.

Lord Advocate: Are you recommended to use plumbago?—Yes.

You were concerned with the Edison Telephone Co. from its formation till its amalgamation with the complainers?—Yes.

And have now no connection with the telephone?—No connection.

Lord M'Laren: Was gas carbon used?—Yes.

Lord M'Laren: Were you an electrician?—Yes; or as is here known as an engineer. I assisted Mr. Edison in his experiments, but only voluntarily, as my work was chiefly to apply.

Lord M'Laren: Was carbon found to be the best for producing the variations?—Many substances were tried, but carbon was found invariably to give the best results.

Lord M'Laren: You have read the English specification. Does it, so far as you know, give the results of the experiments made in the laboratory?—Yes; and the only modifications there made were for the methods of adjustment.

(*This closed the evidence for the complainers.*)

The first witness called for the respondent was Mr. Conrad Cooke, of London.

After the usual deposition—

Mr. Mackintosh: You have made yourself familiar with the various pieces of apparatus in this case?—Yes.

And the Edison patent of 1877?—Yes.

And you were present at the meeting of the Royal Society when Professor Hughes read his paper on the microphone, in 1878; and you also witnessed a number of experiments made by Professor Hughes?—Yes.

You know the second claim in Edison's specification as disclaimed, and you observe he there claims a combination of diaphragm, tympanum, and electric tension regulator as described in the specification?—Yes.

Have you considered whether the instrument known as Theiler's transmitter is a similar or dissimilar instrument to the instrument covered by claim two?—I believe them to be dissimilar.

Do you find that the Theiler transmitter at all resembles any of the electric instruments called by Edison, "tension regulators," in his specification?—No.

Edison's claim includes the use of a tympanum and the use of an electric tension regulator in combination?—Yes.

Now, take the electric tension regulators in the first place. Apart from the tympanum what do you find to be the essential features of the various electric tension regulators described by Edison?—They appear to be all on one principle—depending more or less on the variation of the density.

They proceed by compression of the substance?—Yes.

And by varying the density of the substance?—Yes.

Is there a quantitive relation between the degree of compression and the magnitude of the sound?—I think so.

Do you find there is any virtue in pressure at the points of contact?—No.

From the principles of Edison's tension regulator could you have an instrument dependent upon the variation of the pressure at the point of contact?—No.

Would you just explain why that is so?

By the terms of the specification in almost every instance the device is clearly expressed that it depends upon more or less compression of the mass. In everything he shows that is to be done by the variation of the density of the mass. If you send a current through two conductors with a loose joint there must be a wasting action going on. When a spark passes there is waste, but this is a question of degree, and may be lessened by reducing the power; but there must be a wasting action going on and consequently the pressure could not be constant. The action of a current passing through a joint will cause the two joints to waste away. That disturbing influence would, in my opinion, be so considerable as practically to destroy the working of the instrument.

Now, in regard to the material which Edison used, what do you find to be the essential feature?—It must be compressent, and it must vary its resistance very sensibly by variation of compression.

Is that a feature, which as you read his patent, is common to all the substances which he suggests?—I think so. That I think is expressed by Mr. Edison in his final disclaimer, where he says that the tension regulator increases and decreases its resistance under slight changes of pressure. That feature applies not only to the fibre or plumbago, but to all the substances he suggests. According to Edison there is in his specifications something specified regarding the conductivity of the material used.

What appears to be required as to the conductivity or non-conductivity?—It varies under variations of pressure. He specifies that the material must be either an elastic conductor, or a semi-conductor, and all the materials he suggests are either of the one character or the other. Iron, for example, would not fall within the description of material which he suggests, because it is a hard, incompressible, substance. It is a conductor, but I would not call it an elastic or semi-conductor. It would not occur to any one reading Edison's specification that iron would be used as the substance of an electric tension regulator. I find nothing about gas carbon in Edison's specification. Gas carbon is a pretty well-known form of carbon. The various forms of carbon vary greatly with regard to their properties. The properties of plumbago, for example, do not all correspond with the properties of gas carbon. Gas carbon has many properties which plumbago has not. Differences, in physical property, do not correspond to mere differences of chemical composition. I think the suggestion as to the use of plumbago would not at all convey to me the idea that gas carbon might be used as an equivalent, nor do I think that gas carbon would be substituted for compressed plumbago. I would say the same with regard to lamp-black, which is another form of carbon, or rather another substance into the composition of which carbon enters largely.

Now that being Edison's instrument, would you look at Theiler's instrument? How would you describe its principle?—It is a form of Hughes' microphone, and devised by Hughes. I was present at the Royal Society when Professor Hughes published his description of the microphone. That was regarded as a very important discovery indeed by scientific men, and it has excited a great deal of attention since in the scientific world. It has introduced a new means of research altogether. It introduced quite a new principle into the construction of transmitters. Various forms of transmitters have been since constructed and used according to Professor Hughes' system, and have been used commercially successfully. The Crossley transmitter is a modification of Professor Hughes' system, and so is the Gower-Bell and others, and all these instruments are in extensive use commercially. Before Professor Hughes introduced his microphone, only the magneto, or Bell's form of telephone, had come into use in this country. It was not in very extensive use, but it was coming on. Professor Hughes' discovery made a very great stride indeed towards the commercial use of the telephone.

Then, what do you take to be the principles of Hughes' microphone?—The first and most striking principle is a shaking and variable contact between the two parts constituting the microphone. That shaking and variable contact is produced by the movable portion being affected by sounds. There are no mechanical means employed for producing that shaking or vibration.

What material does Professor Hughes use?—Almost any conductor will do.

But how did he first arrange his material so as to produce this loose or shaking contact?—The first experiment I saw of Hughes' gave five or six different forms. One of these was the form they had in Theiler's transmitter—two carbon pencils with a third pencil forming a bridge between them and resting loosely upon them. The only difference was that this was a vertical position. In Theiler's the bridge or cross-trees is on the two lower pencils in the vertical position, while in Professor Hughes' experiment there were two of these pencils placed side by side, and one across the top, what he called the "log hut" principle.

Was that at all different from the arrangement which Theiler now employs?—No; only in position. In my opinion that instrument does not act by pressure upon the mass of the transmitter. I have made experiments in order to test whether or not it is possible that it can so act. The object of the experiments was to ascertain whether gas carbon was capable of having its resistance varied by slight influences of pressure, and the result was that gas carbon differs so entirely from what Edison describes in his specifications as the properties of the substances he employs, that, instead of varying enormously, there was probably no sensible change of resistance whatever, even when enormous pressure was put upon it—just up to the crushing strain. That showed that under Hughes' system, where gas carbon was used, the instruments could not possibly work upon the principle of pressure.

These experiments were more than one—would you be good enough to describe the first of them?—I took a small pencil of gas carbon similar to what is used by Theiler, and twisting the wire tightly round each end so as to get a really good contact with no shaking at all, and placing the two ends in a vice, I found that there was no change whatever in the strength of the current passed through them when the greatest strain was put upon it, until just at the moment of crushing, when it began to give way, and showed a great deflection immediately. I used a galvanometer, a battery, and a vice. I put the point of contact into perfect contact with the conductor, and put a strain on where the current was flowing through the carbons, there being no loose joint at all. That, in my opinion, was a quite satisfactory experiment; but I wished to verify it, and I tried an experiment in two other forms. As my microphonic experiments did not always affect the galvanometer, I made two experiments which I may explain in this manner. I took a pencil of gas carbon, a very small solid block of gas carbon, another pencil of gas carbon, and then another block. In the first experiment the two ends of the carbons were connected with a telephone in a distant room. These two ends were in an intermediate block, and independent of the other block, and were connected with the telephone in the other room. Taking a little mallet and hitting or tapping the top block, I was putting on a rapid pressure—that is to say, the pressure was communicated through, and there was not the slightest sign to be detected when the wires were connected with the intermediate block—the wires passing through the block at the top and bottom. On repeating the experiment, in doing which the wires were still left on these blocks, but loose, and connecting the wires of the telephone to the top and bottom blocks, so that the current had to pass through the two blocks, a very slight tap was now repeated almost as loudly as the sound made by the tapping.

The Court: Quite as audibly as ordinarily by the telephone.

Examination continued.

This to my mind very clearly indicates that a microphone made up of this substance varies hardly at all—in fact not at all—by changing the pressure put through it; and that as I get this distinct and very decided transmission of sounds by the tapping, which was much lighter than in the first experiment while the current was passing through the joints, it clearly showed that the question was a surface action between the two. It showed that the action was not the action of pressure throughout the mass, but action of some kind at the points of conduct. In telephonic experiments you often get a loud sound in the telephone transmitted through the microphone, while you get no sound in the microphone.

Then the experiment shows that using carbon with loose contact the change of resistance always depends upon the same action at the point of contact?—Quite so. Where it is a loose or a tight contact it always depends upon some action at the point of contact.

Are scientific men agreed as to what that action is?—Nobody can say one thing and one only. There has been a great deal of speculation among scientific men as to the theory of that action. Various theories have been suggested. The first was this question of the variable shaking joints and even for a theory regarding that there were a great many different opinions. It might be that taking this characteristic type of the microphone—the shaking movement—the movable piece is shaken from one point of certain conductivity to a point of worse conductivity.

THE COURT: You don't adopt that theory?—I don't adopt any one theory. I think a number of these little phenomena may take place together.

EXAMINATION RESUMED.

Another might be that the plate being composed of a rough surface, perhaps cut down by files and perhaps resting upon one or two little points, the shaking might make it jump and the position change to a different affinity. There was also a make and break theory, but the problem is not yet solved, and I don't pin myself to any particular theory. I am quite satisfied of this, however, that it is not pressure in the sense of producing a change of resistance. Of course there must be pressure between the two things, but beyond that I do not think that pressure has anything to do with it. I have seen other materials than carbon used in the microphone transmitter. Iron nails have been used and have worked quite well, they were clean nails, not rusted—bright French nails—and the same phenomena resulted there. They transmitted articulate speech, I have myself heard it.

In Edison's instrument, according to his principles, could you transmit articulate speech by means of nails?—Oh, no; I used no diaphragm in the experiments made with the microphone. I have witnessed numerous experiments in which articulate speech was conveyed by the microphone without using diaphragms—in fact, none of Hughes' use the diaphragm to convey articulate speech. Taking carbon, or any other material, I do not think that Edison's system as described in the specification of his invention would work without the use of the tympanum. The tympanum is essentially part of his system. I have seen in use an instrument on the microphone principle known as a cork transmitter. It was very simple. There were the two plates of gas carbon with holes in them, and a little plug loosely fitted into these holes, connecting the two holes by loose contact, and then they had the cork below and above, and wires from the battery passing through the two carbon plates. I have seen that transmitter used, and it has produced quite distinctly articulate sounds.

Have you any doubt or do you know what the effect would be upon the resistance of an electric current in plumbago if operated upon by pressure in the same way as you operated upon gas carbon?—I have made no experiment; but I should say it must break down. It will alter much quicker than the other one.

Have you seen nails used, in the place of carbon pencils, without any diaphragm?—Yes, and articulate sound is produced even with the nails. They are a very good transmitter. These experiments have been pretty frequently repeated in my presence. I was present when Professor Hughes showed his invention, for the first time, before four people, and I have since that time seen them repeated, I can fairly say, at least one hundred times.

Is there any identity of principle between the microphone or the Theiler modification of it, and the Edison transmitter?—No, there is not.

Is the mode of working the same or different comparing Edison's with Hughes' and Theiler's?—Yes; Edison's must have a diaphragm, and the others can do without the diaphragm. In using the microphone the use of cork is of some importance, because it is so very sensitive. The smallest sound is rendered loud in the receiver, and a loud voice speaking near the sensitive instrument will produce a roar. If you muffle it in any way you prevent that roar and render articulation more distinct, and cork is used for that purpose. As an electrician I had never seen nor heard of such results in the transmission of articulate sounds before Hughes introduced his instrument—so loud or so distinct. The Bell transmitter was very distinct, but it was feeble; but taking the loudness and distinctness together Hughes' microphone produced quite new results.

Do you find anything in Edison's specifications pointing to delicacy of surface contact as being the principle of his instrument?—The term "surface contact" is mentioned in one clause "intimacy of surface contact." That was all he saw about it.

What do you understand it to mean?—I do not understand it at all.

Do you take it as at all suggesting the principle of the microphone?—It does not suggest anything to me. It is very ill defined. As an electrician I am sure I could not have constructed an instrument from the description given in the specification. It says something about what would result from a disc covered with plumbago. Now, in the first instance, I would require to know what the disc is made of, and whether it is a conductor or a non-conductor. It shows that he uses semi-conductors made up of insulating materials. Then he says "covered with plumbago," which might, or might not, be a cake of plumbago—it might be only painted on, or rubbed on, or it might be simply a polished surface giving merely the finest film. But taking the idea that pervades the patent that it is a question of compression, he does not at all indicate how it is applied. Witness, proceeding, quoted the phrase: "proximity or extent of surface contact." It seems to me, he went on to state, quite impossible to understand any of this. There is no instrument in the terms of that passage shown in the drawings, and no other reference to any instrument of the kind of construction.

And if those discs were to be covered with plumbago, and then compressed more or less one with another, would not that be just a case of pressure over the mass?—I do not understand the construction sufficiently to answer the question.

Would there be anything inconsistent with the bits of description given in the specification that it was pressure over the mass?—I could not even answer that.

Are diaphragm and tympanum just the same thing in scientific language?—The tympanum is diaphragm, but it does not follow that the diaphragm is tympanum. The tympanum, as I understand, is a thin, flat membrane instead of tissue, whereas the diaphragm is a thin sheet of metal.

Now, what is implied by fixing it round its circumference?—The tympan in any recognised sense of the word is always, I think, held by it edge tight, and freely moves from the centre.

You could not have the tympan, the whole surface of which was glued to another body?—It would cease to be a tympan in that case, and ceasing to be a tympan it ceases to be diaphragm in Edison's sense.

THE COURT: I mean more than a verbal distinction in this matter between my tympan and the one described in the patent.

MR. MACKINTOSH: Edison used the two terms tympan and diaphragm as convertible terms, and I want to bring out the difference.

EXAMINATION CONTINUED.

The tympan could not act unless part of its surface is free and the edges are held down?—It could not act as a tympan. If it were glued down over the whole surface he would not call it a tympan.

THE COURT: Because it would not vibrate in that case at all, or it would vibrate in a different manner?—I mean that the tympan is held round its edge, and under the influences of the vibration, it vibrates in the thin part in the centre; and if you glue it to a board all over it ceases to be a tympan at once.

EXAMINATION CONTINUED.

Would an iron plate glued over its whole surface to another body act as tympan acts?—It would vibrate.

On the principle of forced vibration or of push and pull?—On the principle of force. It appears to me that the whole object of Edison's tympan or diaphragm is a machine for increasing the push and pull produced by the sound of the voice. I find that throughout Edison's specification the term tympan or diaphragm is always used in the sense which I have described and as having the action I have described as the peculiar action of the tympan, showing that if this great invention embodied the microphone, one of the greatest discoveries, he would not put it into his claim and couple it with the diaphragm. It could be used without the diaphragm.

In answer to the Court Mr. Mackintosh said that his case was that they did not use the telephone, but the microphone, which was a cognate invention.

EXAMINATION CONTINUED.

WITNESS said: I have examined figure 1 accurately and find the diaphragm in it connected the same as in the other instrument—fastened round its circumference and free over its surface excepting where it is pressing on the tension regulator. The cork piece fastened in the centre does not interfere in the least with its freedom of action except when it loads it a little and makes it vibrate a little slower.

Does the cork dominate the diaphragm, or the diaphragm the cork?—The diaphragm the cork, of course.

Does the diaphragm in Edison's specification ever operate by shaking?—I would consider it vibrating.

Does it operate on the tension regulator by shaking, or does it always operate by compression?—By compression. It never operates by shaking. I understand that vibration might cause shaking. It does not operate by any shaking at all similar to the shaking produced by the microphone. It operates by the push and pull action. I have said that the tympan might be dispensed with. There is nothing corresponding to it in the microphone.

Is there anything that produces motion by push and pull?—Nothing but sounds in the air, and they do not operate by push and pull.

In Theiler's transmitter there is a piece of cork and a piece of iron plate fastened to the cork. Have you considered whether that would be in a proper sense a diaphragm or a tympanum?—No, it seems to be the reverse of what Edison means by it. The cork appears to be a semi-insulating substance placed between the point of sound and the microphone. The cork does not appear to me to be designed to serve the purpose of the diaphragm, nor does it, in fact, serve the purpose of the diaphragm, but rather produces the reverse result. No part of the iron plate is free in the model shown.

Does the metal plate perform any function important to the action of the instrument?—It preserves it from moisture and gives a finished appearance to it.

THE COURT: Has it no acoustic influences?—I think not.

EXAMINATION CONTINUED.

Can it have any acoustic influences placed as it is?—Well, every portion of the instrument must have some acoustic influences; but the cork, whether lined or not, will increase or decrease the effect. Here the cork dominates the plate.

Have you any opinion how the Theiler instrument would act if the iron plate were away?—I do not think it would make any material difference at all, except that it might get out of order more readily by moisture, dirt, and various influences. I do not consider that the action of the instrument would be affected were the cork away also. The experiments I have made with the microphone led me to this conclusion. Professor Hughes' first microphone for transmission of speech he had shut up in a soft box, concealing it all over.

How would you describe the motion communicated in the Theiler transmitter on the carbon pencil by or through the iron plate or cork?—I do not think they go through that any more than any other part of the case. I think the sound waves find their way to microphone by sympathetic vibration, but if sound waves are made in the vicinity every portion of this instrument takes up a portion of their effect—

cork, iron, wood, cover, and everything just in proportion to the extent of the surface presented by each of these.

In working the Edison instrument do you find any adjustment suggested or required as between the tension regulator and the diaphragm?—That is an essential feature.

Is the tension regulator always screwed up more or less tightly against the diaphragm?—There is always contact between the two. There are arrangements in the specification for screwing up the tension regulator and the diaphragm. That is not aimed at at all in the microphone or in Theiler's transmitter.

What is the object aimed at in the microphone or Theiler's transmitter as regards the contact?—To have a variable surface contact between the two, and not too sensitive; and to have it as loose as possible, consistent with preserving the continuity.

What is the advantage of gas carbon over iron?—One advantage would be that gas carbon keeps perfectly clean. With regard to the carbon, as oxide is either dissipated as gas, or does not interfere with the coal surface, except the reason I have given there is no other for preferring carbon to iron in the matter.

Might platinum be used quite as well as carbon in the microphone? —I do not think quite as well, but it might be used quite as effectually.

Coming to the receiver, you know the receiver which forms the subject of Morgan-Brown's claims?—Yes; it consists of an electro-magnet operating by attraction upon an iron disc in its neighbourhood, and worked by a single battery in the line. Morgan-Brown's patent was taken out in December, 1876. The idea was not entirely novel in December, 1876. There had been a good deal of discussion in scientific journals upon the subject of the telephone and improvements on the telephone prior to that date, and various forms of receiver were well known to scientific men, and to persons who took an interest in the subject. Amongst these there were the Bryce receiver, and Gray's and Lacour's, and also Graham Bell's, or the membrane telephone. In the last-named they had the electro-magnet, with a single battery in the line. A disc of goldbeaters' skin, with a small armature glued to it, was situated close to, just in front of the electro-magnet, of which the magnet operated by attraction. That was in use before 1876. It was published in America in August; but in September, 1876, the Graham Bell instrument was known in England, and descriptions of it had been published. In September, 1876, the British Association met in Glasgow, and in the mathematical and physical section of the Association Sir William Thomson presided. I was present and heard an exposition which Sir William gave of an improved telephone, which he explained he had brought over from America. A report was published of Sir William Thomson's address, taken, I believe, from the verbatim notes of a shorthand writer. From the information I obtained at that meeting I published an article in *Engineering* of December 22nd, 1876. Except the information I got at that meeting I had no other information on which to proceed in preparing that article; but besides hearing his address I had had some conversation with Sir William Thomson on the subject. I asked him a question or two. Sir William had a couple of instruments on the lecture table beside him. They are produced in this case. He illustrated his exposition by references to them. They were left on the table when his address was over, and remained on the table all day, and were there all the week, during which the meetings continued, open to the inspection of members. I made drawings of the instruments at the time, and these drawings were afterwards published in my article in *Engineering*. These drawings were sent to the *Journal of Engineering* before the Brown patent, but all the information which these drawings conveyed was information communicated to me at the British Association meeting. Interrogated as to the features of the receiver which Sir William Thomson exhibited, witness deposed: it contained an electro-magnet, but the instrument was not shown in operation. There was no suggestion of any local battery, none but the line one.

Did it occur to you at the time that there was anything new in the substitution of the disc for the membrane previously in use?—I observed the difference between the transmitter and receiver—iron in place of the membrane.

Was anything said about the position of the iron disc towards the rest of the instrument?—Yes. Sir William described it as vibrating and producing the sound, and I asked him in what way it operated, and he illustrated it by saying that when the instrument is at work the disc is held firmly down. I asked him the question at the close of the lecture. I might imagine that it was by magnetic action. I took that as one of the most wonderful things I had ever seen in my life.

Was it in accordance with your own experience that the magnetic action of the instrument would be sufficient for the purpose?—I suppose so. I have since experimented to see whether the attraction is sufficient to hold such a lid as that firmly down when the instrument is at work, and I find that the moment the current is turned on this will be attractive. The moment the current goes on the disc goes down by itself automatically. I have made the experiment both with this particular instrument (Sir W. Thomson's) and with a copy of it which I had made.

You found that in the copy of the instrument produced that the moment the current is applied the disc came down and remained down while the machine was in action?—Yes; I made the same experiment with Sir W. Thomson's model, but the same thing did not happen. The disc did not go down, and it struck me that either it required a more powerful current than my copy of the model, or that there was some defect in the apparatus itself. I made an experiment to find out how it was, and I found that there was actually no current at all. There was a defect in the connections, which would account for its not working and not holding the lid down.

[At this stage the witness illustrated what he had been saying by pointing out the defect in the machine before him.]

In that state of matters did it surprise you that Sir Wm. Thomson's instrument would not work and would not have its lid pressed down when in action?—Certainly not. It could not.

Have you tried whether your model or copy that holds down will act as a receiver?—Yes. The articulation is not so distinct. The Graham Bell is not the form now in general use. My copy did transmit articulate sounds.

Would you have had any difficulty in making the necessary connections with the instrument which Sir Wm. Thomson showed?—I would have seen first what was wrong with the instrument. I found in the published statements a description by Bell of his whole detail. It is from the journal of the *Telegraph*, August 1st, 1876, and it gives the resistance coils and to a great extent tells how to make the instrument. It also specifies the number of elements, and the moment they put on the battery, as described by Bell, the disc must go.

In what respect does the Morgan-Brown receiver differ from the instrument shown in Glasgow by Sir Wm. Thomson when in operation?—Only in the method of holding the diaphragm.

How is it held down in the Morgan-Brown patent?—Mechanically screwed together between two circular frames in place of being held down by the action of the magnet. That does not appear to me to be a substantial difference, and it has no advantage except simplicity.

Is it in respect of what took place at Glasgow, coupled with the previous statements regarding it, that you hold that plan to have been anticipated?—Yes.

What are the essential features of the phonograph?—It is really what is called Edison's talking machine—a machine which will reproduce sounds by mechanical means, so that it includes the elements of recording and reproducing. But the element of reproducing is the particular point, and it is after an interval of time or immediately after the sounds are uttered. I have studied the provisional specification of Edison. It does not state the nature of the invention. There is no indication whatever in the provisional specification of the reproduction of sounds by the phonograph.

Is there anything in the petitioners' specification which appears to you to describe the nature of the invention claimed in the third claim? —No; it describes portions of the apparatus as being interchangeable—available in transmitting and recording; other portions for local use; some only available for transmitting; others only available for receiving. It strikes me that the word "recording" here is really a misprint for receiving. Common sense seems to say that it means for transmitting and receiving. Then there is a line which says that some portions can record. That, I believe, he really meant; but it was to make some sort of registration of the vibrations taking place in some portion of the apparatus. But Mr. Barlow prepared an instrument long ago, called the logograph, which made the drawings —phono-autographs—a very similar arrangement, in which the vibrations of the diaphragm caused another sort of record; and, again, Professor Graham Bell, in all his inventions before made use of an instrument like the human ear, so that all these are equally applicable to the question of making a record of atmospheric sounds. The mere recording, which is not the thing claimed in law, has been anticipated, and therefore in reading the provisional specification you are to consider the probabilities of the patentee having intended merely to claim something which had been claimed previously, for recording, as distinguished from the phonograph, was really old. I find nothing in the specification indicating that Mr. Edison had in his mind the reproduction of the sound as claimed.

But now, taking Edison's transmitters as described in these drawings, do you know of any one of these having been in commercial use?—Not one.

THE COURT: The interesting part of this inquiry relates to the mode of action of the carbons in what is called by the one party the transmitter which you have in the microphone. Can you tell me when two points in an electric circuit are separated, but separated only by an infinitesimal space, whether the current is entirely interrupted or only weakened?—It would depend on the strength of the current and the distance the carbons were separated.

Is it possible when in the action of the microphone the circuit is broken, but broken only by being separated by such a very small space, that the current may continue with varying intensity according to the distance of separation?—Yes; quite probable.

Is that one mode of action which may account for the variation of this instrument?—I could not possibly separate one cause in the action of the microphone from a large number.

The reason I ask the question is that we had evidence yesterday to the effect that wherever the circuit was broken there can no longer be transmission of the vibrations in fac-simile, because there would be a positive interruption in the current and instead of a continuous propagation of undulations you would have a succession of broken undulations. The object of my inquiry is whether if the separation be very small or infinitesimal, there might not be an actual breaking of the current or merely a diminution of intensity?—I do not agree with that view that the interrupted current—that is, in an infinitesimal degree, as your lordship states—is detrimental to the telephone. In the Bell transmitter you get an interrupted current because the inducting diaphragm passes a neutral point at which there is no current whatever, and in moving one way you produce a positive, and in moving the other way you get a negative current.

Then the only question I have to ask is whether the variations we have been speaking of might not be fairly described as greater or less "intimacy of contact?"—Yes, they might in that sense.

Then I understand you think that the passage in the specification where the phrase "intimacy of contact" is used is not a sufficient description of the mode in which action is made to enable you to make a machine?—No, my lord. The only mode of interpreting the clause is to take it with the context.

EXAMINATION CONTINUED.

Assuming the latent (?) break, however infinitesimal, would it be the maintenance of a closed circuit?—No, it would not be a circuit; but if there was a very rapid vibration the break would not affect the working of the telephone.

(At this stage the Court adjourned for luncheon.)

On resuming Mr. Cooke was again put into the witness box and cross-examined by the Lord Advocate.

Interrogated: Tell me if I rightly understood your idea as to the *rationale* of Edison's invention. I think you said that in all the methods, as you read the patent or the specification therein described, the variation of resistance is produced by the variation in density caused by compression?—Yes, I think so.

Your notion is that Edison claims no modification in which the resistance is not varied by the compression of what I may call the mass?—Just so.

And the compression of air affects the density of the mass?—That is so. That is my reading of it.

And I think you said that so interpreting Edison's patent the action of the apparatus does not depend upon the application of pressure to the point of contact?—Except so far as connected to the mass?

Except so far as it means the pressing the mass, and therefore increasing the density of the mass?—Yes; but I do not consider there is any virtue in Edison's invention by compression at the point of contact.

As regards the apparatus of Theiler you consider that that is substantially Hughes', and you consider that Hughes' is distinct from Edison's in this respect—that he does not get his results by producing compression in the mass, but by producing contact between two surfaces—what you call the points of contact?—In the form of microphone there is not. Clerac introduced the powder tube into one form of the apparatus.

Am I correct in saying that your notion of distinguishing Edison from Hughes involves the theory that Edison's apparatus works according to a certain scientific method, being that of compression producing density in the mass and thereby causing a variation in the current?—Yes; whereas in the other I think there is nothing of these.

Edison purports to patent a certain apparatus?—That is so.

He does not profess to patent any principle or theory, but simply a certain apparatus?—No.

What is it in Edison's patent that you contest? Is it the limitation of apparatus working only on a certain scientific theory or application?—Well, perhaps it is. The words used in the first paragraph, such as, "intermediate semi-contact," "electric tension regulator," were passages in which he is describing certain modifications of his apparatus.

I think you said that on Edison's principle you could not get a variation of pressure at the point of contact?—I say this, that in all Edison's apparatus he makes surface contact as good as it can be, so as not to vary.

I do not find he says anything about there being a necessary proportion of compression and the results of action of sounds, or that he limits himself to cases where there is such a proportion?—There is a passage to that effect. There are marked passages in which I find the law of proportion limiting Edison's invention.

You do not find that law of proportion expressed in any of the passages you quoted?—Not expressed, but it sets forth how a given material will comport itself under pressure, and it is from such passages that I deduce a general law. There are various materials specified by Edison, and they possess the qualities varying in degrees.

In the passages you have already read contact is described in Edison's as being material "where the springs are adjusted to make contact?"—Yes.

Then there is also this expression, "proximity or extent of surface contact." Do you see from that that in Edison's view the area of surface contact was material?—I do not see that.

Do you see from that that one of the two alternatives was proximity or extent of surface contact?—Yes.

Does not that show you that Edison had in his mind two surfaces being in contact over a greater or less area might vary the degree of resistance to the transmission of the current?—I do not read it that way.

But now that your attention is called to it what would you say?—The compression would not affect it unless it were elastic.

Is it the case that there would be a greater or less degree of resistance presented according as there is greater or less surface contact presented?—Yes.

If you presented two larger areas to each other there would be less resistance than if the areas were smaller?—Yes.

And it would be reduced to a minimum if the contact was reduced to a point?—Yes.

Even if you did not increase the pressure would you diminsh the resistance, if you increased the areas of contact?—Yes.

And that is indicated by the mode of working it out?—It is indicated by Mr. Edison in the specification.

Upon what ground do you say that Edison limits himself to that law of compression when you find that he indicates the alternative mode of producing the same thing although he has not shown its working out?—The only way I can understend that working at all is by supposing this surface to be a thick elastic surface and compressible, and that when compressed together the areas would spread out and increase to that extent. If they are not elastic the areas will not increase.

In the case of the Theiler instrument, is the effect of the diaphragm spoken of such as to move the vertical pencil against the tranverse pencils?—I do not think it is—whichever direction the cork takes the pencils will follow.

Is it not the case that when you speak against the diaphragm or tympan you cause a movement of the pencils in the direction of the vocal waves?—Yes, but not necessarily from them.

Does that not create a pressure on those vertical pencils against the lateral pencils?—I do not think so.

Is the lateral pencil moved at all by speaking?—Yes; by the sound waves striking on it.

Is there no such action as I have described in the case?—That I am not prepared to say.

What is the object of the brass backing?—It is to reduce its sensitiveness to prevent it being kicked too easily away.

What would kick it away?—The sound vibrations. It is quite immaterial to Theiler whether the cork and the tympan are flexible. It would be just as good for his instrument to have it like the rest of the box.

Is your view that the lateral pencil is moved by the motion indicated indifferently through all parts of the box?—Yes.

What is the use of the mouth-piece at all then?—I think it is a superfluity.

A ridiculous superfluity?—Yes.

And is the diaphragm a superfluity?—I do not consider it a diaphragm at all.

Then it might be just a box without any breaks at all?—It would do as good.

And better, perhaps?—I do not know that.

You having been retained for the respondent are at this moment unable to conceive any reason why he used a box with a mouth-piece and with a diaphragm upon it?—Well, it might be to preserve the outward form of transmitters in general. I believe that is the real object of it.

And that is described as "Theiler & Sons, London"?—Yes. He has a patent.

(Shown Theiler's specification for improvements in telephonic and transmitting and receiving apparatus.)

The LORD ADVOCATE read the following extract from the specification:—"The first part of new invention relates to an improved method of constructing telephonic transmitters, in which the pressure of two contacts against one another is made to vary by the vibrations f a diaphragm or sounding board."

CROSS-EXAMINATION CONTINUED.

Do you see from Theiler's provisional specification that he attached importance to the diaphragm?—Yes; he mentions it the first thing.

Is it not by the vibrations of the diaphragm that he varies the relations of the carbon pencils to each other?—Yes.

It is not merely getting it through the walls of a box?—No.

Then do you still disagree with Theiler as to the utility of the diaphragm?—Entirely. I think he is wrong in attaching importance to the diaphragm.

In your judgment is he wrong in the view that it is the movement of the diaphragm which makes the contact closer or less close?—He is wrong in attributing that to it, yet that is the only action of it.

But is Theiler wrong in attributing to the movement of the diaphragm the aggregation of loosening of the carbon?—I think so.

No such action takes place?—I do not say so.

Do you think it possible that air waves can be sent without moving the pencils inside?—No. I cannot say that.

They are moved inwards?—I do not say inwards.

Well, are they moved in the same direction as the vocal waves?—I am not prepared to say that. They are set into tremor.

Suppose there is a single note spoken; that drives the air against the tympan and moves it inwards?—Not moves it inwards; it shakes it inwards or outwards.

It must first go forwards?—I do not say that. The first movement of the tympanum might be towards the mouth. I am not aware that the common theory is the other way.

In the case of Edison or Bell is there any inward movement of the tympan by the air waves?—Yes; but the question of whether it is inward or outward will be the same in both.

Perhaps you could kindly assume for one or two questions that it might be that the tympanum is moved inwards by the vocal wave; well, assuming that, it would of course press the vertical pencils inwards?—Yes.

And by so doing would press these pencils against the lateral pencils across them?—Yes.

And I suppose the vertical pencils would not move the lateral pencils until the pressure was so strong as to remove the inertia of the lateral pencil with its brass backing?—Yes. That would require considerable pressure, and the particles of the lateral and vertical pencils would be squeezed together more than before the pressure.

And there would be a greater extent of superficial contact?—There might, or there might not be.

You seem to be in a state of most philosophical doubt about everything. Do you not know whether carbon is elastic?—It has a certain amount of elasticity.

Is it compressible?—Yes; all substances are compressible; it is a question of degree.

Therefore, when you put a pressure on the outer pencils so strong as to overcome the inertia of the inner pencil, with its brass backing, you squeeze so as to flatten it out slightly?—It might be, but that is a minute philosophical question.

If I squeeze my two hands I flatten them *pro tempo*?—Quite so.

Have you not to some extent, it may be greater or less, a resulting presentation of larger carbon surfaces to each other?—Yes.

Have you not also a greater or less increase of the density of the carbon bodies?—I should think it would be very minute indeed.

If that is so then don't you get both the things in Theiler's apparatus which Edison claims? You admit you get greater extent of surface contact?—Yes.

Greater proximity when you squeeze?—Yes.

You commentated on the word "proximity" as used by Edison. Do not you get a greater amount of that proximity when you squeeze the two together?—I say I don't understand that remark.

You have everything in Theiler that you have in Edison, although in different degrees?—It is a very different thing.

You have the same elements producing the result of varying the resistance to the electric current?—Yes.

And the variation of the resistance to the electric current is the thing which determines the transmission of all the vocal notes?—Yes.

You told us that in your view Theiler's instrument is simply the microphone—the microphone has nothing to do with the reproducing but with the magnifying of sound. Is not that so?—It is rather the transmission of sound. It renders audible by a large sound a very minute vibration.

But when you use the microphone as it is used here, as a transmitter of vocal sounds, it does not make these sounds louder?—No.

Then is it performing any function there except the transmission of sound in the apparatus we are in question about?—It has the transmission of electric currents.

Does Hughes attach any importance to pressure as producing contact?—He did for awhile, but I think he has dropped it.

Just as you have dropped Theiler's diaphragm?—I think the theory he gave at the Royal Society was not the one he accepted or holds now.

Was it too like Edison's?—Well, it was very similar.

Is it not just the getting of a larger number of points of contact by pressing the carbons so as to squeeze and so far flatten them?—Yes.

Hughes understood proximity, also, we see, from the specification. Does he still stick to this theory?—I never heard him repeat it.

Has he ever withdrawn it?—He has always explained his phenomena by the more accepted theory of shaking contact.

Shaking involves moving?—Yes.

Can you make a thing shake except by moving, or, in the case of these carbons, do you make them shake otherwise than by the wind; don't you, even for a time at all events, bring them into the close pressure described by Hughes with the consequence described in common by both Hughes and Edison?—Both causes contribute to the same result in the cases of Hughes and Edison.

We had the phrase "jolting contact" yesterday: would you prefer that to shaking contact?—Yes.

The Court: I do not reject the supposition that there may be a very minute separation of the current circuit.

Cross-examination resumed.

I do not reject in the case of Hughes a partial increase of density.

You have enumerated different causes which co-operate in different degrees; do you accept this as one, that an elastic form will yield when interfered with, and that in consequence of the yielding an increase of surface contact will result?—Yes.

And there will be a diminution of the infinitesimal distance which may before have existed between the parts—that is what Edison calls proximity in his pamphlet and what Hughes calls proximity in his address?—I do not understand the question of proximity with reference to this of Edison's, but the expansion of Hughes' is not, if I remember rightly, in this form of microphone that we are considering, but in his tubical form containing powder.

(*To be continued.*)

JUDGMENT.

Lord M'Laren gave judgment in this case on the 1st instant. It is that the complainers are entitled to protection by interdict under Edison's, as well as Bell's (or Morgan-Brown's) patent.

We shall publish Lord M'Laren's review of the whole case, in full, in due course.

We intend to publish early next week the complete report of this case, including judgment in detail. Price 6d., by post 7d.

UNSCIENTIFIC ELECTRIC LIGHTING.

Doubtless the readers of this journal have observed that our recent article on "Unscientific Electric Lighting" has been noticed in the addresses delivered by the chairman and vice-chairman of the Anglo-American Brush Electric Light Corporation at the first annual meeting of that company. We are glad to see that the speakers had evidently taken this matter to heart, but one of them mistook the spirit in which it was written. The vice-chairman, Mr. Sellon, declared that it was intended as a diatribe against their system, and that we unfairly and wilfully perverted facts. We imagine that Mr. Sellon had not perused the article in question in his cooler moments, and that his imagination got so far the better of his judgment as to upset his rational ideas and to make him the actual perverter of facts, instead of ourselves. If he will condescend to glance through our columns again he will find that other failures than those of the Brush system are mentioned, and also that we do not say one word against the system he advocates, our remarks being entirely confined to the *application* of electric lighting. Indeed, we commenced by commenting upon the excellent machines and lamps at present in use, and it is to this fact that the present success of the electric light may be ascribed. We did not even go so far as to inveigh against conducting wires being suspended from lamp to lamp, and we hope Mr. Sellon will give us credit for believing that underground wires would scarcely be put down for a three months' trial on the chance of taking them up again at the end of that time; but what we did state we now repeat, to the effect that had these overhead wires, as used in Edinburgh, been properly insulated and suspended, and every precaution taken to secure an uninterrupted passage for the electric current, this failure would not have given us the occasion for the observations which are apparently so distasteful to some of the directors of the Brush Company. The address of the chairman, Sir Henry W. Tyler (who, by the way, we inadvertently in our report of the meeting called Mr. Henry Tyler), was much more temperate in its character, for he did not try to hide these unfortunate failures, but rather brought them out, and evidently understood the reason of such little accidents. He admitted the want of suitable assistants, and put this down as one of the principal reasons of mishap. We think, however, that such men might be obtained, when it becomes thoroughly understood how necessary it is that work of this kind should in its infancy be done well, for the future of any undertaking very much depends upon its application in the present. The superintendence of such installations should, at any rate, be in the hands of professional electricians. Sir Henry has certainly taken our remarks in a more kindly spirit than his vice-chairman, for he bears out our meaning entirely when he says:—"But in the failures it is not the system that has failed, but the application of it—it has been simply some stupid little mistake in applying it." His remarks generally are very much to the point, but we cannot agree with him without much further proof than we have at present that the company possesses the best systems of lighting both by the arc and by incandescence. Neither can we join issue with Mr. Sellon that their dynamo machines are superior to any others. That their installations of the electric light are far ahead of other competitors, we are free to admit; but this we think is as much due to the competitors themselves as to the system used or energy displayed by the Brush Company. The great attraction in the Brush system is the number of lamps that may be placed on one circuit, aided in a smaller degree by the lamps themselves, and the appearance of the machines. If competing companies would only reconstruct their machines to give, say, twenty lights in one circuit, we must confess that in our opinion they would stand a much better chance of competing on equal terms than they do at present.

We do not, however, altogether advocate the use of *a large* number of arc lamps in one circuit, for various reasons. In the first place, in spite of Sir Henry Tyler's ingenious way of showing how little danger is to be apprehended in the use of the Brush system, we scarcely need say that such high tension machines are dangerous to those dealing with them, and that greater care must be exercised than is necessary in the case of gas. Again, should the main circuit be broken all the lamps must necessarily fail. But, as before mentioned, the fact of being able to put twenty, thirty, or forty lights in one circuit is a great attraction, and carries great weight with corporations of towns, local vestrymen, &c., and as it is necessary in almost every business or profession to consult the tastes or ideas of those with whom contracts are made, it would be well if other manufacturers of machines and lamps launched out a little more on the lines of the Anglo-American Brush Electric Light Corporation.

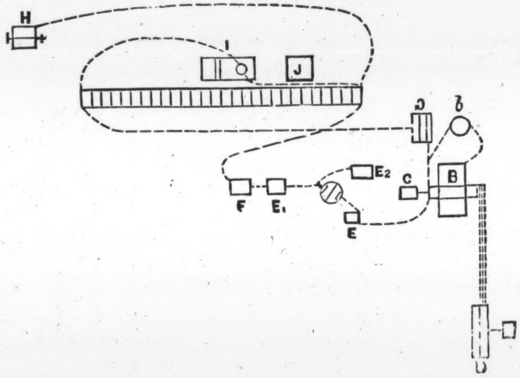
That this company should have sold in England 220 dynamo-machines, and 2,711 arc lights, is in itself a remarkable result considering the short period of time during which its operations have been carried on; and the fact of having now on hand 4,000 orders seems to promise a success in the future, not only for this particular system, but for others as well, of a kind that will soon establish electric lighting generally as the agent for any illumination of an important character all over the country. This great success on the part of the Brush Company has already brought forth results that might have been foreseen, for another large company has been floated for carrying on the business of electric lighting, employing for such purpose the apparatus of the Brush system. We hope that in a short time the companies which contributed so greatly to the early working of the electric light will likewise have their share of prosperity, for large sums of money must have been lavishly expended by those who, in the face of so many difficulties, pluckily kept plodding along, and whose efforts have added so much to our present knowledge on this subject.

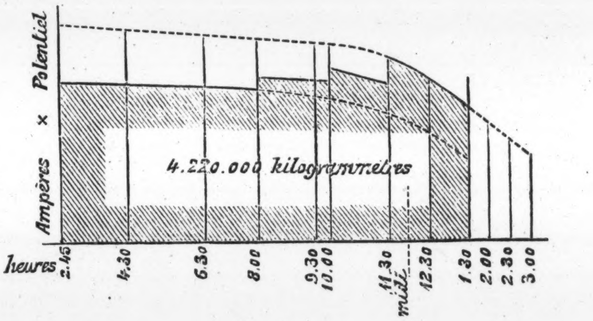
THE FAURE ACCUMULATORS.

SOME very interesting experiments have recently been made with these accumulators at the Conservatoire des Arts et Métiers, at Paris. These experiments, conducted solely under the direction of a committee as distinguished as the commission of experiments at the Electrical Exhibition, in a place so well suited to the purpose as the machine room at the Conservatoire des Arts et Métiers, have had every advantage which might render them as conclusive as possible. They lasted more than five days, and we may say that the most important properties of these accumulators were examined, such as their capability of preserving the stored-up energy and the amount of work effected by them. We shall see that the figures and results of these experiments agree as nearly as possible with those given by the eminent electrician, Sir William Thomson, several months ago.

The members of the commission who personally attended at these experiments were MM. Tresca (the president), Potier, Joubert, and Allard, with the assistance of M. G. Tresca. M. Gérard watched the experiments as the representative of M. Belpaire.

The accumulators, thirty-five in number, were of the laboratory type; they were round, with an outer stone jar, the exterior diameter being 25 centimetres, the total height 35 centimetres, and the total weight about 30 kilogrammes, with the water.





The electrical resistance in a model of this type is somewhat high, and this causes a loss of power at the charge and discharge, somewhat greater than there would be in the machines practically used, such as those employed on tramways, where it is necessary that the time occupied in charging and discharging should be comparatively short. It is interesting to know the reason of this difference, for we shall then be able to reconcile results of experiments which appear widely different.

Our readers probably know that the Faure accumulator, unlike all the secondary batteries known, consists principally of two conducting supports, on which are disposed the materials, which, by the ulterior process of affinity, are to become active materials, namely, oxide of lead, spongy lead, &c., whereas M. Planté's battery, for example, is rendered active by the disintegration of the plates themselves, which, in M. Faure's apparatus, are only supports.

Now, M. Faure, having found that the surface of these supports, while exercising a great influence on the resistance of the battery exerts none whatever on the absolute capability of storage—which depends solely on the thickness of the spongy lead applied to the surface of the supports, and these points depending on the cost price—the result was that models of great and small resistances were constructed.

The round accumulators, which form the subject of our article, were formed of two plates wound round in a spiral. The positive plate—*i.e.*, the one bearing the negative terminal—was a millimetre in thickness, and the negative one two millimetres; both were perforated with holes of one centimetre, and had received on each surface a coating of red lead in the proportion of 10 kilogrammes to a square metre.

We give a diagram showing the arrangement of the instruments employed in this experiment, which began on Wednesday, Jan. 4th, at 10 o'clock in the morning.

However, before that time, a preliminary experiment had already taken place, consisting in discharging the batteries of what they might accidentally contain. This was done on the preceding day, and also during a part of the morning, occupying altogether about six hours, during which a loss of power was observed towards the end with an average current, the unit adopted being the power at the extremes of 35 elements in a series, and the luminous intensity being also noted.

This last experiment was somewhat necessary, for the object was to measure not the total capability of storage, but the capability—or rather the result—between the practical limits during which the lamps should give a satisfactory and uniform light.

These preparatory measurements gave the limit of the experiment, which was besides very well determined as to its termination, by the rapid decrease of luminous intensity.

As the above figure shows, the movement was transmitted to the Siemens machine by means of absolute dynamometer, the readings of which, calculated in the usual measurements, are given in the accompanying table.

The Siemens machine having been found to produce a higher power than that required, it was necessary to introduce a resistance into the exciting current, which not being absolutely requisite in this system, should be deducted from the total labour expended.

The figure explains sufficiently the object of the different instruments; we need only add that the deposit of copper in the ampèremeter, G, always corresponded with the indications given by the other instruments.

The electromotive force of an accumulator when charged was found to be 2·15 volts, and in reading the figures of the potential column obtained on the Thomson potentiometer we must remember that this figure of 2·15 corresponds to 32·5, and also that these figures, by the arrangement of the graduation, are in direct proportion to the tangents of the angles.

A few slight corrections will be required in the figures that we give in the following table; but as this would take a certain time, and as besides a report will be published by the commission with all desirable details, we do not pretend to quote them as being absolutely exact; they express the average of the results within the limits that the time at our disposal allows us to obtain.

TABLE GIVING THE WORK OF CHARGING.

No. of Hours.	Total Work in Kilogram-metres.	Work per sec. in Kilogram-metres.	Total current. Ampère-Hours.	Average current.	Potential indication.	
5·30	2·420,000	122	67	12·18	36	Wednesday.
7·30	2·730,000	108	50	7·15	39	Thursday.
7·30	2·920,000	111	59	7·89	39	Friday.
2·45	1·130,000	109	17·5	6·48	39	Saturday.
22·45	9·200,000	112·2	193·5	8·5	37·75	Average.

The resistance mentioned above as having been introduced into the exciting circuit of the dynamo machine in consequence of the too feeble resistance of the electros was measured, and the average of work thus uselessly expended was estimated at 10 kilogrammetres, so that the average work per second would be 102·2 kilogrammetres.

The average potential reading of 37·75 corresponds to 87 volts for the battery of 35 elements charged, which gives an average of effective electrical work of 74·5 kilogrammetres for labour expended by the machine of 102·2 kilogrammetres, and consequently a result of 71 per cent. This result might evidently have been better, as is shown by the great speed imparted to the machine, which made on an average 1,060 revolutions, notwithstanding the small amount of work required.

The process of discharging the battery was performed in two operations with an interval of 36 hours between. It was begun at 2.45 o'clock on Saturday, January 7th, and continued until 10 o'clock at night, then resumed on the Monday morning at 10 o'clock, and finished at 3; but virtually it ended at 1.30, according to the luminous indications and others, as we have explained at the beginning of this article. During the discharge the desire of imitating experimentally what takes place in practice, with the object of rendering uniform the luminous or other result obtained, led to a slight apparent confusion, if we may be allowed the expression; but if the theoretical study should suffer somewhat, the practical result obtained is most interesting and important. The battery was discharged in the first place, commencing with 30 elements, and adding the five others in succession with the evident object of rendering uniform the result; this was done easily and in a very satisfactory manner simply by working the commutator.

In the same way, in the last discharge, an increase was made from 30 to 32, and then to 35 elements.

It is, besides, very easy to find out by calculation what would be the curve of this discharge if the 35 elements had been employed at the commencement; we show it by a dotted line in the second figure.

During the discharge the instruments were disposed in the same manner as indicated above for the charge. There were in addition 11 Maxim lamps, one of which was placed in a dark cabinet, with the photometric apparatus of the commission and under the care alternately of MM. Allard and Leblanc.

Here again it is our duty to say that the figures in the following table are not to be taken as absolute; but they give very close approximations to the absolute results that will certainly be obtained:—

TABLE OF THE WORK OF DISCHARGING 35 FAURE ACCUMULATORS INTO 11 MAXIM LAMPS.

Time of the Day.	Current Ampères.	Potential Indications.	Observations.
2 h. 43	0	27·8	30 discharging elements.
2 45	16·37	26·2	
4 30	16·4	26	
6 30	15·68	25·5	32 elements.
8 30	15·49	25·3	
8 5	16·70	27·2	
9 30	16·24	26·5	32 elements, Monday.
10 0	16·06	26	
10 0	0	28·5	
10 15	16·82	26·9	35 elements.
11 30	16·20	25·7	
11 35	17·74	27·9	
12 30	16·42	26	The principal experiment finished.
1 30	14·73	23·9	
2 0	13·85	22·8	
2 30	13·26	21·9	
3 0	12·64	21	

All that we now have to add to these figures, which speak for themselves, is the number of ampère-hours calculated to have been rendered during the legitimate duration of the discharge, which amounts to about 178, giving a result in current of $\frac{178}{193}$ or 92 per cent. five days after the charge.

The work which is represented by the whole shaded surface of the diagram amounts to 4·220,000 kilogrammetres.

Moreover, we had put into the battery work represented by an average current of 8·5 ampères per second, during 22 hours 45 minutes, or an average power of

$$\frac{2{\cdot}15 \times 35 \times 37{\cdot}75}{32{\cdot}5} \quad 87{\cdot}5 \text{ volts.}$$

which represents work to the amount of

$$\frac{87{\cdot}5 \times 22{\cdot}75 \times 3{\cdot}600 \times 8{\cdot}5}{g} = 6{\cdot}020{,}000 \text{ kilogrammetres,}$$

the accumulator therefore gave

$$\frac{4{\cdot}220{,}000}{6{\cdot}020{,}000} \times 100 = 70 \text{ per cent.}$$

We have seen before that the rendering of the Siemens machine, under the very bad conditions in which it was placed, was also 70 per cent., so that even under these circumstances the result in light nearly attained 50 per cent. of the mechanical labour at first expended.

In conclusion, one of the most important results of the experiment is that the 35 accumulators, weighing altogether about one ton, gave 110 kilogrammetres of work on an average during 11 hours, or more than 16 effective horse-power hours.—*L'Electricité*, Jan. 28th, 1882.

CORRESPONDENCE.

VYLE'S EASILY-TESTED LIGHTNING CONDUCTORS.

To the Editor of THE TELEGRAPHIC JOURNAL.

SIR,—Before replying to the special point which you have invited me to deal with in your footnote to Mr. Cox-Walker's letter of last week, permit me to mention briefly the work to be gone through before the person desirous of testing a lightning conductor on a two-storeyed house would have the testing ready wherewith to commence his special duty. I recently tested the conductors on two houses in Cleveland, both of which, though large, were two-storeyed. The services of a handy man being obtained, he had to search for a ladder long enough to reach to the roof. Two men were needed to carry and raise it. Taking a smaller ladder in his hand, my muscular assistant mounted to the roof, and from the apex thereof to the top of the conductor. Here with a file he made clean the rod, and attached the testing wire thereto, dropping the other end down for my use. There was yet, however, an earth to be made, and an attachment to be fixed to the conductor at the ground line. The earth was found in a water pipe one hundred yards distant. I then took a test of the metallic circuit formed by the testing wire to the top, the conductor, and the wire from the latter at the ground line to the instrument. Noting the result, I took off the lower wire, and substituted the earth connection. As I had been fortunate in getting a good earth from the water pipe, the difference between the two results, when allowance had been made for the length of wire to the water pipe, was not so good as I had anticipated. The actual time occupied with the tests, after all was ready, was but a few minutes, no one should be a better judge than yourself of that. The wires were then taken off, and the ladders removed and taken away, fully two hours having been occupied thereby, and no time lost. Take another case. When the British Association met at York, last September, I was invited by Mr. W. H. Preece, F.R.S., on behalf of the members of the Lightning Rod Conference who were present, to test the conductors on the York Cathedral. Having got the necessary material, &c., on the spot, two linemen were sent to the top of the central tower, with a ball of string to throw out clear of the battlements, and to it was attached the testing wire, which they drew up. The conductor having been filed clean, Mr. Preece, who had gone to the top to superintend the work there, placed a telephone in circuit and spoke, so as to save shouting. In the meantime matters had been got ready below, and the method of working being agreed upon, the telephone was taken out of circuit and the tests taken—the metallic one as described above and the other to earth. Another conductor, on a distant part of the minster, was also tested, the two occupying three hours, as separate earth was needed. Five persons were engaged in the work, which, had one of my new form of conductor been used, with its tiny test-box, could have been done in as many minutes for each as it occupied hours for both. But had either of the conductors been upon a tall spire—instead of within easy reach by stairs—how would the test have been taken? Only at great cost, risk to life, and excessive waste of time. It is to obviate this cost, risk, and waste, that my conductor is valuable. Taking a hint from our French neighbours, I have adopted a trembling bell, which, being rung first through a given resistance, with one or two chloride of silver cells, is so arranged that by moving a switch, it then rings through the conductor, if of sufficiently low resistance. Failing to ring—due allowance being made for leakage—would imply a higher resistance, and when a fair margin is allowed beyond the proper resistance, this should be a very simple and cheap testing apparatus.

As regards the "spirelets" to which you specially referred as a difficulty, I must confess I do not so look at it, as it will be very easy to make metallic attachments to the conductor, by soldering a strip of copper to one of the strands of the conductor, and so connecting the "spirelet" or intermediate point, reserving for the last point on chimney the connection between the testing wire and the conductor proper.

I had looked forward to the Report of the Lightning Rod Conference for *details as to methods of testing*, and notwithstanding the careful collection of evidence from all parts and parties, and the recommendation *to test conductors*, there is not a word said as to the "how" of doing it, which to me, at least, is a matter of disappointment.

Apologising for the length of my letter, and pleased that in the improved form your journal is opening its pages to discuss this subject, I am, Sir,

22, Borough Road, Yours faithfully,
Middlesbro', SAMUEL VYLE.
Jan. 25, 1882.

To the Editor of The Telegraphic Journal.

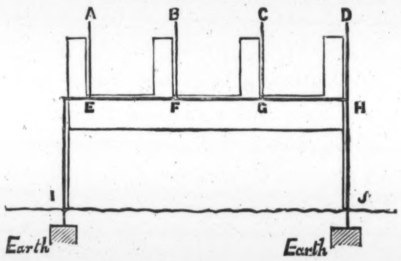
Dear Sir,—I notice with pleasure the discussion going on in your columns on the subject of lightning conductors, and I quite approve of the comments you have made, in answer to your various correspondents. It seems to me that there is at least always the possibility, if not the probability, of the inside wire of Vyle's lightning conductor becoming short-circuited with the outside strand. Although the former is not insulated with gutta-percha, which of course would easily be melted by the heat of the covering strand in summer, I can still imagine the short-circuiting to take place anywhere between the test-box and the point, from such causes as defects in manufacture which might not show up when tested in the works, but which might be developed in the fixing to a house, or chimney shaft, and also by the action of the lightning itself. Suppose such a fault as a short circuit occurred near the top of the strand, and suppose that the inside wire had actually parted connection with the point from some reason or other, then a test on this lightning conductor would still apparently show that everything was in proper condition. I cannot imagine that Mr. Vyle ever intended using a Wheatstone bridge and a particularly sensitive galvanometer for testing such work, but this would be necessary if one wished to know whether the inside wire was insulated up to its connection with the point, or had a short circuit near. Your latest correspondent, Mr. E. Cox-Walker, would oblige by giving a description of the way in which he tests these conductors, and I would ask if the plan adopted by Messrs. Siemens Brothers is not superior to that of Vyle's. The point is connected to a double strand, one running down one side of the house and the other coming down in an opposite direction, or both might be used side by side, but slightly parted, and one fixing might do for both wires.

Of course in using a double wire the respective strands would be of such section as to give the combined conductivity of the present larger single strand, and I would suggest that each strand had a separate earth-plate. One strand could be parted at the test-box and connected by a terminal, which could be opened when a test is required. The lightning conductor of such a form could not only be tested electrically, but it would also be visible throughout its length if the wires were brought down side by side, so that should a fault occur it could be seen.

It appears to me that the real object of testing a lightning conductor is to find out if the earth connection is good, and that after all if this was so the ordinary form of conductor would answer every purpose, for surely the point can be made absolutely certain of metallic contact with the strand. The most useful improvement in this matter, to my way of thinking, would be to make a really good and sure earth for the termination of the conductor. I have often thought that an adjunct of great utility and at the same time of but little expense, would be to place a pipe in the ground from the earth-plate to about three feet up the wall, the exterior termination of the pipe to be a metal basin in which water could be emptied now and then by servants or anybody connected with the house or building. This, of course, would be only intended for such places as are not permanently wet. Now that the discussion is fairly started, I trust that others may give us the benefit of their opinions, and also that those who like Mr. Cox-Walker have had practical experience in fixing conductors will give the results of what they have observed in the course of their labours. Hoping that you can find space for this letter, which has rather exceeded the length I intended, I remain, &c.,

Jan. 27, 1882. ELECTRICIAN.

[We certainly think that Mr. Vyle has made out a very good case in favour of his improved lightning conductor, even when employed for two-storey houses; the value of the invention as regards lofty buildings, church spires, &c., we did not question. Mr. Vyle's reply with reference to our remarks concerning "spirelets" does not appear clear. What we wished to have explained was the following:—The sketch here given represents a building with chimneys or spirelets, each of which has a conductor, A E, B F, C G, D H, attached to the main conductor, I E F G H J. Now it must be evident that no test will be complete unless it proves the continuity of the portions, A E, B F, C G, D H, and also the soundness of their junction with the main conductor, I E F G H J. It seems to us that to prove these particular points it will be necessary to have test wires from each of the points, A, B, C, D. "Electrician's" remarks with reference to Siemens' double conductor for chimneys seem to be of considerable weight.—Edit. Elec. Rev.]



To the Editor of The Telegraphic Journal.

Sir,—I am glad you have invited discussion in your valuable columns upon Lightning Conductors. Messrs. Cox-Walker speak of Mr. Vyle's patent conductor being very easily tested, and I may add my humble testimony as to the value of the simplicity in this most vital point, thus placing in the hands of the uninitiated a sure and ready method of determining the value of each conductor without the aid of a qualified electrician, and when more generally known will, no doubt, be universally adopted.

I am located in the centre of a large colliery district, and it is simply astonishing to see the utter disregard exhibited for lightning conductors: large collieries and even public places of worship being absolutely unprotected, the former offering a very ready path for the discharge of lightning through the head gearing, the wheels and wire ropes of which are in a bright condition from continual use, thus affording a direct circuit for lightning down the shaft head. If true data could be ascertained there is no doubt that a number of colliery explosions could be traced to the absence of a good protector at the pit heads.

I hope, Mr. Editor, that this discussion will be thoroughly taken up, and not be considered too insignificant by men of eminence, as it is of the utmost importance to the community at large that every means should be adopted to call public attention to this subject.

Yours respectfully,
WEBER.

Barnsley, *Jan.* 25*th*, 1882.

B.—Will answer in our next.

NOTES.

Electric Light in Russia.—St. Petersburg, Jan. 22 (Evening).—The authorities have decided as an experiment to light the Newsky Prospect from the Anitschkoff Palace to the Moscow street by electricity. The contract is to run until 1886.

Underground Cables.—The telegraph pole and wire nuisance which prevails in all cities will probably soon be abated to a great extent. In New York and Chicago the wires are rapidly being put underground, and it is possible that the time is coming when the underground method of telegraphing will be in vogue all over the country, as it is now pretty generally in Germany. In Chicago four miles of this cable have been laid and the work is being pushed as fast as possible. Underground telegraphing has been a problem difficult to solve, and it is only recently that it has been brought to anything like perfection. The method in use at Chicago consists, in brief, of a cable made up of No. 12 copper wire, inclosed in a lead pipe, resting on a covered pine trough, and sunk into a trench three feet deep. The preparation of the cable is thus detailed: Each wire is first run through a glass tube an eighth of an inch in diameter. Twenty of these wires, each incased in its glass tube, are inserted in a lead pipe. One end of the latter is then tightly closed by slipping over it an air-tight "cap." A small opening is cut into the upper surface of the pipe, and the cut piece of lead turned back. It now remains to make the tube thoroughly air-tight and completely insulate the copper wires, and the way it is done is as unique as it is interesting. A hot solution of resin, tallow, bees-wax, and other insulating compounds is poured into the pipe at its open end. As the hot solution flows through the pipe it drives the air before it, and takes the place formerly occupied by the atmospheric fluid. The air, unable to find an exit at the closed end of the pipe, is forced up through the small opening cut in its upper surface. The solution continues to be poured in until the air is finally entirely expelled, when the lead cutting on the upper surface is thrown back into place, filling up the opening, and an exterior application of solder prevents the entrance of any air from the outside. The result is an air-tight lead pipe, filled with copper wire incased in glass tubes, and the whole arrangement is thoroughly insulated by the solution of hot resin, bees-wax, tallow, &c. The ends of the copper wires, however, are allowed to extend several inches beyond the lead pipe, in order, of course, that two connecting pipes, with insulated wires, may be properly joined together, and the line drawn out by the union of its several sections. The process of making this wire in every detail is interesting, and involves great care and nicety of execution, but when completed, turns out a pipe full of thoroughly insulated wires, and renders a break in the electric current impossible. The manufacturers of this wire are said to be crowded with orders, showing that the telegraph companies contemplate putting it into use at once in the cities, where the removal of the unsightly poles and wires is very desirable.—*Kansas City Journal.*

The Society of Telegraph Engineers.—On the evening of February 9th, a paper on the "Pilsen" arc light will be read by Mr. H. F. Joel, C.E. This will be copiously illustrated with diagrams, and we believe the hall of the institution will be illuminated by this system of electric lighting.

Jay Gould's New Cable.—The New York *Sun* of Jan. 11th, 1882, says, "Telegraphic communication between North and South America, which has been carried on in a very roundabout way, is to be made more direct under a scheme originating with Jay Gould, who is at the head of the American Cable Company. It has been known that Mr. Gould sought to get a concession for landing a cable at Brazil, and the Rio de Janeiro *Jornal do Commercio,* of Dec. 6th, announces that Mr. Gould's plan is to lay a cable through the Bermudas to St. Thomas, thence in unequal curves to Paramaribo, in Dutch Guiana, and from that point to the city of Fortaleza, the capital of the province of Ceará, in Brazil. The cable has been ordered, and the Brazilians are waiting anxiously for the completion of the line.

"Communication with Brazil and the South American Republics is now carried on by way of England, and the cost is about 3 dols. a word to Rio de Janeiro. For some time the Brazilians have been dissatisfied with the service of the English companies, and when, last summer, Mr. Gould applied to the Brazilian Government for privileges equal to those the English companies enjoy, he was fortunate in hitting upon a time when the English companies were particularly aggressive in trying to force communication over their cables to Buenos Ayres instead of over the land lines. His cable will shorten the distance between New York and Rio de Janeiro more than two-thirds, and the Brazilians will not be confined to the use of one line.

"Besides this, telegraphic communication between New York and South American republics will also be established by way of Vera Cruz and Panama. The Central and South American Telegraph Company is pushing the construction of its land lines and laying the cables as rapidly as wires are supplied.

"There is to be a telegraphic convention in Buenos Ayres during the exhibition there next month, and the indications are that the Emperor of Brazil will attend, in order to promote a better understanding of the telegraph business in the republics. The land lines of Brazil end at Ceará, where Mr. Gould's cable is to land, and the desire is to have the land lines under a perfect system for better communication with the United States."

Electrical Railways.—We learn from our contemporary *La Lumière Electrique* that a most interesting experiment of running two cars at once was made under the immediate superintendence of Dr. W. Siemens on the electrical tramway working between Berlin and Lichterfelde. The cars were both of the ordinary patterns used on this line and of which one figured in the German section of the late Electrical Exhibition held at Paris. When one car was on the line a second started from the terminus, got into movement with the utmost ease and rapidity; it was only at the engine working the dynamo machines that any difference was observed, here it at once became evident that about double the power was being developed while the two cars were running simultaneously.

Electric Light in Paris.—We believe that it is shortly contemplated to light by means of the electric light, the large and splendid central market, or Halles, in Paris. As by far the greatest business activity takes place at such very early hours as to be carried on, especially in winter, during darkness, this will be a great boon to all the provision merchants. The light is to be paid for at per hour and per intensity of light developed. This will be certainly one of the most useful and practical employments that the electric light has yet been put to.

Electric Light at the Crystal Palace.—On Wednesday evening, the 25th ult., the first trial of 6 Pilsen arc lamps and 10 Joel semi-incandescent took place; by the end of the week these will be continually at work. The Joel system will be supplemented shortly by 20 or 30 additional lamps designed expressly for and in character with the Pompeian Court. For supplying the current to these, Messrs Rowatt and Fyfe will employ one of E. S. Hindley's 6 horse-power engines and two more Siemens D^2 machines. A novelty will be also exhibited by this firm in the shape of a horizontal Pilsen lamp, which we believe has not yet been seen in England, together with the lamp of MM. Sedlacek and Wikulill, a description of which we gave in our issue of the 14th inst, and which has been used in France and Austria as a headlight for locomotives.

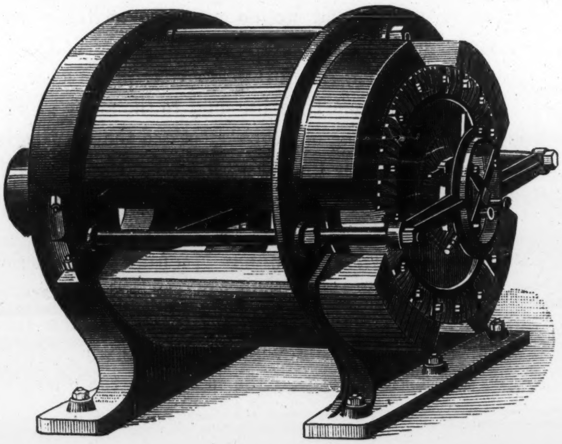
The Hunnings' Cox-Walker Telephone.—We are requested by Messrs. Harrison, Cox-Walker and Co., of Darlington, to state that we have been misinformed as to our announcement last week that Messrs. Barraud & Lund were acting as agents for their telephones in London and that they had them now at work between their places of business.

"A Telephonic Concert.—A private rehearsal or trial performance in connection with the Crystal Palace telephonic concerts was given on Saturday at Sydenham, the music of the grand organ being conveyed from the interior of the organ in the Handel orchestra to Ribblesdale House, the residence of the manager, Major Flood-Page, two or three furlongs away in Crystal Palace Park. The transmitter was placed in the organ, and one wire used to carry the sound to Ribblesdale House, where it was distributed among six sets of receivers, so that half-a-dozen people at once could listen with both ears to the music. In this way Gounod's 'Funeral March of the Marionette,' Mendelssohn's 'Wedding March,' and other pieces were conveyed to all the listeners at once, the higher passages coming with sharper and more agreeable distinctness than the lower, but all being clearly and pleasantly rendered, so that a practised organist could dissect the music and say what stops were being used. Occasionally other sounds were transferred, the movement of the feet of the people in the neighbourhood of the transmitter, the pulling out of a stop, &c. A remarkable and novel effect was noticed in the simultaneous conveyance of spoken sounds, together with and above those of the music. Side by side with the Ribblesdale House transmitter was another one in connection by the same wire with the house of Colonel Gouraud, about one mile and a half on the other side of the Palace. Messages to Mrs. Gouraud were sent through the telephone while it was receiving the musical sounds, and her replies could be distinctly heard through the music. This result, which is analogous to the duplex telegraphic system, offers the prospect of remarkable developments in the application of the telephone. It was felt by the select circle of acquaintances invited by Major Flood-Page to his house on Saturday afternoon that a really important secret had been discovered, and that the possibility of hearing concerts and operatic performances in our own houses is being brought speedily closer and closer to the stay-at-home Britons."

A short time since it will be remembered the performance of "La Mascotte" was heard through the telephone at some distance from the place of performance. The music (instrumental) and voices were distinguished quite distinctly. This was also the case with the opera performances in Paris during the Electrical Exhibition, so that what important secret was discovered during the telephonic concert at the Crystal Palace we are at a loss to understand. The question of bringing stay-at-home Britons into connection with the concert-room by means of the telephone is not new, and many experiments have been made with this idea. It is well know that the telephone is capable of taking up and reproducing vibrations of however complex a nature, and we do not see anything more wonderful in the *remarkable and novel effects* noticed on Saturday last than if A and B were conversing in a concert-room and at the same time listening to the music; and if one hears the shuffling of people's feet and the drawing out of organ stops whilst the music is going on, it is hardly surprising that a voice could also be distinguished as well.

The Jurgensen and Lorenz Machine.—M. Guerout, in commenting on this machine in our contemporary, *La Lumière Electrique*, says:—"What constitutes the really original part of the Jurgensen and Lorenz machine is firstly, the electro-magnet on the interior of the armature; secondly, the particular mode of winding the wires of the inductors, and the strong ventilation which the rotation of the armature provokes. This last point, as well as the empty spaces left between the wires of the electro-magnets is advantageous because it lessens the heating of the machine. The concentration of magnetism in consequence of the coil on the polar appendages, is also not without advantages, but the rounded form given to the inductors does not seem to us very logical so far as construction is concerned. From this point of view also we shall criticise the manner in which the interior inductors are supported, being maintained only by one extremity of their axis. In this position the weight of the electro-magnets must tend to bend the support and to put the machine out of order. Besides, these interior electro-magnets had already been employed by M. Siemens, but with an armature coiled with wire on the outside only, and MM. Jurgensen and Lorenz maintain that the presence of the wire in the interior of the ring considerably increases the inductive action in proportion to the action of the interior magnets; we do not, however, think that this is the case. It seems to us that the induction is produced principally by the pole developed in the revolving ring of iron by the inductors, and it matters little whether this pole is caused by the outer electro-magnets alone, or by inductors placed on the interior and exterior. This theory is confirmed by the fact that Fein's machine (see *La Lumière Electrique*, July 13th, 1881), which also possesses interior induction, has not given better results than the Gramme machine. As to the Jurgensen and Lorenz machine, as far as we know no experiments have been made with it at the Exhibition, by which we might judge of its practical value. The analogy which exists between it and the Fein machine, as regards the manner in which the ring is influenced by the inductors, leads us to think that it will give results little different to those obtained from the latter."

MAGNETO-ELECTRIC MACHINE, WITH CONTINUOUS CURRENT, OF M. DE MERITENS.—The permanent magnets form four groups, each consisting of sixty-four plates of steel a millimetre in thickness, collected on a framework of bronze. Their extremities projecting from the front part of the framework, present four cylindrical surfaces, in the centre of which the ring turns. The ring also contains sixteen induction coils; but the plates of the cores are cut so as to form four projections, on which the coiled wire forms four distinct spirals. These coils are mounted in the same manner round a bronze wheel. They are connected in a series like those of a Gramme ring, and shunt circuits are established at each junction to connect them with the sixty-four plates of the collection. The employment of six alternate magnetic areas involves two plates of commutation, placed like those we have met with in the octagonal machine of M. Gramme. There are thus four rubbers, two for each of the currents collected simultaneously, currents which can be utilised either separately or together. The collector is mounted on the core of the bronze wheel, which avoids all displacement of the wires when the machine is taken to pieces. A movable bronze ring serves to support the axles of the rubber-holders, and facilitates the exact regulation of the points of contact of the rubbers on the collector. This arrangement also allows of reversing the rubbers when the machine is required to be used as an electric motor.



THE STORAGE OF ELECTRICITY.—We extract the following excellent letter from our contemporary, *Nature*, January 26th:—

> We have heard a great deal of late in reference to what is called the storing of electricity, and not long since we had a long account in the *Times*, of the journey from Paris to Scotland of a gentleman who carried with him a number of cells "filled with electricity," and representing "hundreds of thousands of foot-pounds of force." The daily papers and the scientific serials have vied with each other in telling how electricity can be stored, or bottled up and transported from place to place, to be drawn upon as circumstances may demand.
>
> The result is that the majority of those practically acquainted with the subject have very false ideas as to the nature of the Planté, the Faure, or the Sutton accumulators. In no sense of the word can these beautiful forms of batteries be called storers of electricity. A man who should carry with him a piece of copper, a piece of zinc, and a little sulphuric acid, and should then boast that he was transporting electricity from place to place, or carrying half-a-dozen thunder storms in his pocket, would be rightly regarded as committing an abuse of language. A man who carries a box of lucifer matches in his pocket has no right to say he is transporting fire from place to place, or to speak of them as storers or accumulators of fire. In like manner it is an abuse of language to speak of electricity being carried from place to place, or stored up for future use in the Faure secondary battery. Nor is it less incorrect, or less misleading, to speak of "charging" such batteries with electricity. The dynamo machine may render the amalgamated lead and copper of a Sutton battery capable of being unequally acted upon by sulphuric acid, and of thus giving rise to an energetic current of electricity, and the reversing action of such batteries is undoubtedly very beautiful and certain to be of the greatest possible practical convenience, but there is nothing in the principle of their action to justify the very misleading language used in reference to them, not only by writers to the provincial press, but by scientific men in high class journals. Practical electricians understand generally perfectly well what they mean by the figurative language they use, but it would be well, if in lectures and articles of a didactic nature, or intended for the information of the general public, they were to use language of a less metaphysical character and to describe a thing as it really is. It is because as a teacher I know how apt people are to give a concrete significance to abstract or figurative expressions that I ask you to find room in your pages for this short protest.
>
> EDMUND P. TOY.
>
> Middle Class Schools, Littlehampton, *January 13th*.

CABLE NOTES.—We understand that the Société Générale des Téléphones in Paris have bought the works of Messrs. Rattier and Company, at Bezons (Seine-et-Oise), and that they intend manufacturing submarine and subterranean cables.

M. Richard, late Directeur Ingénieur of the French Government Telegraphs, and one of the representatives of France at the Telegraph Conference, held in London in 1879, has been appointed Engineer-in-chief of these works.

It is rumoured that the price of the works, paid by the Telephone Company, is £200,000, and that they will probably have some connection with the Marseilles-Tunis cable, which the French Government intend laying this year.

WE are informed that the *Dacia*, belonging to the Silvertown Telegraph Works, successfully landed, on the 25th instant, the shore-end at Chorillos, of the Chorillos-Payta section of the Central and South America Telegraph Company's cables.

NEW COMPANIES.—HAMMOND ELECTRIC LIGHT AND POWER.—Objects: To purchase the electrical business of Robert Hammond (trading as Hammond and Co.) and the benefit of certain licences granted to him by the Anglo-American Brush Electric Light Corporation, and to work the same. Purchase consideration £35,000, in shares of the company, and 20 founders' shares, entitling him to a two-fifth share in the profits of the company after the payment of a 10 per cent. dividend. Capital £250,000, in 49,980

£5 shares, and 20 founders' shares of £5 each. Signatories (with one share each): Sir C. G. Young, Bart., 5, Ashburn Place, S.W.; J. J. Courtenay, 1, Essex Court, Temple, E.C.; W. Low, 34, Billiter Street, E.C.; R. Cobb, 11, Lime Street; Colonel F. G. Steuart, 14, St. James' Square, S.W.; F. Barker, Woodford; G. Logan, 14, Barnard Street, W.C. R. Hammond is also a director. Qualification: the holding of 100 shares. Remuneration: £1,000 per annum and one-tenth of the profits over the first 10 per cent. dividend. Registered January 20th, 1882, by Messrs. Ingledew and Ince, St. Bennet Chambers, Fenchurch Street, E.C.

DOMESTIC ELECTRIC LIGHTING.—Objects: To carry on the business of an electric lighting company in all its branches. Capital £50,000, in £1 shares, 49,000 of which are entitled to 10 per cent. per annum dividend payable out of profits, and one-fourth of the surplus profits after such dividend, and 1,000 entitled to the balance of profit Signatories (with one share each): W. Byrne, 69, Sydner Road, Stoke Newington; W. G. Card, Mallinson Road Wandsworth; C. Cooper, 4, Amelia Terrace, Leytonstone; M. Davis, 37, Little Queen Street, Lincoln's-in-fields; T. J. Vincent, 18, Prospect Road, Walthamstow; T. J. Porter, 18, Broughton Road, Stoke Newington; W. R. Waller, 94 Sussex Road, Holloway, N. Directing qualification: the holding of 50 shares. Remuneration: not less than £1,200 per annum. Registered January 18th, 1882, by Mr. F. Clift, 111, Cheapside, E.C.

LONDON AND GLOBE TELEPHONE AND MAINTENANCE.—Objects: To construct and carry into operation an invention for improvements in electrical apparatus for the reproduction of sound, letters patent for which have been granted to W. R. Lake. Capital £600,000, in £10 shares. Signatories (with one share each): W. Shepherd, Woodgreen, N.; T. Sorrey, 39, Abingdon Villas, S.W.; C. W. C. A. Gaillard, 36, de Laune Street, Kennington, S.E.; E. Dion Lane, 39, Lavender Road, Clapham Junction; E. M. Turner Brockley; H. Sampson, 14, Nightingale Road, Lower Clapton; G. R. Hazewell, 22, Cecil Street, Strand. Directing qualification: the holding of fifty shares. Remuneration: £1,500 per annum and 20 per cent. of the net profits after the payment of a dividend of 10 per cent. Registered January 23rd, 1882, by Messrs. Davidson and Morriss, Queen Victoria Street, E.C.

THE CRYSTAL PALACE EXHIBITION.—On Tuesday evening last the Lord Mayor of London visited the Crystal Palace for the purpose of awarding the prizes gained by exhibitors at the late Wool Exhibition. These were distributed by the Lord Mayor in the Concert Room, which was lighted by the incandescent system of Edison. We believe that Major Flood-Page, the Manager of the Crystal Palace Company, sent official notices to all exhibitors, stating that the exhibition was to be considered as fairly started from that day. There is also, we are informed, an idea of making the exhibit a permanent one. A large muster of the general public was noticeable, and a number of members of the electrical profession were also congregated there. The electric-lighting arrangements are not by any means complete, but amongst the systems at work, or attempting to work, on the occasion we refer to, we may mention those of Rowatt & Fyfe, now permanently fitted up, and showing each evening seven Pilsen arc lamps, and ten Joel lamps of the well-known semi-incandescent type. These are apparently in first class working order, and the former lamp will probably produce in England as favourable an impression as was accorded it in Paris. The 6 Crompton lamps in the theatre, with the current supplied by two Bürgin machines, did not do very well, but any little defects will doubtless be rectified at once. The Edison incandescent light made a fine display, a magnificent chandelier in the small theatre being especially noticeable; there were, however, about 10 per cent. of these little lamps *hors de combat*. The Mackenzie lamps (Strode & Co.) four in number, and each worked from a separate Siemens machine, all driven by a gas-engine, were continually going out, and the general effect was of course unsatisfactory.

The 32 Brush lamps (16 from each machine) kept up their usual standard of efficiency, although at times there was an unpleasant hissing. The Hammond Electric Light Company also exhibited Brush lamps.

The British Electric Light Company were unfortunate in being unable to keep the Brockie lamps alight at all.

The Electric Light and Power Generator Company with the Weston lamps were fairly good, these having been in operation for some weeks past. The Maxim incandescent light of this company was employed for illuminating the banquetting room, where Mr. McGeorge, the chairman of the Crystal Palace Company, entertained the Lord Mayor. When everything is in complete working order the general effect will be good, but by that time the evenings will be naturally so light that it will necessitate visitors staying very late if they wish to examine the various competing systems.

CABLE NEWS.—The managing director of the Indo-European Telegraph Company notified on the 27th January that the submarine cable between Lowestoft and Germany had been repaired, and that this company's direct route with the East was restored and re-opened for traffic. We have to congratulate Mr. Bordeaux, the energetic engineer of the Submarine Telegraph Company, who has been fortunate enough to repair this cable twice since the middle of October last.

That the work is of no ordinary difficulty is proved by the fact that the Telegraph Construction and Maintenance Co., first with the *Kangaroo*, and afterwards with the *Stormcock*, have been unable thus far to repair the German Union cable, which was broken early in October last, and prior to the first interruption of the Indo-European cable. It may be mentioned that during this time these are not the only two repairs made by the *Lady Carmichael*, and after the great public cry of the inefficiency of the arrangements made between the Post-office and the Submarine Telegraph Co. for the maintenance of this line and the Zandvoort cables, we think it only justice to the latter company to point out the success of their staff in this matter. It has been said by no less an authority than Sir James Anderson that any boatswain could lay a cable, and this we can almost believe from the success of late years of the Telegraph Construction and Maintenance Co. in laying submarine cables. Their lack of fortune in repairing cables in the North Sea in twenty fathoms of water, and the fact of their abandoning other repairs, would apparently show that some special ability is wanted to carry out submarine telegraph work.

The repair of the Brest-St. Pierre cable, which costs the Anglo-American Company a considerable sum of money, was not, from an engineering point of view, a success, as the quantity of cable reported to be used in these operations, we believe 70 or 80 miles, was about four times the amount which might reasonably have been expected. But perhaps owing to the repairs being abandoned, after at least one month's work, in the preceding year, there may have been an anxiety to get the cable through at any cost. As a matter of fact, it was noted that the *Scotia* left the river Thames with so little ballast in her that, considering the season of the year, it would have been a miracle had she repaired the Brest-St. Pierre section in November, 1880. Had a little more judgment been exercised the repairs would not have been undertaken, unless with a more suitable vessel, until the following summer. The proof of the correctness of this statement is that the *Scotia* returned to Plymouth, after being a month at work, and reported she was unable to do that which she had been sent out to perform. This mishap cannot be laid to the charge of want of ability in those on board, but rather to a want of ability or discretion in sending the ship to sea. Fortunately, however, the Anglo-American Company did not think as we do, and paid the bill.

Block Signals in France.—One of the last acts of M. Raynal, the French Minister of Public Works, was to issue a circular to the railway companies, directing them to apply the block system on double lines absolutely instead of permissively, as in the majority of networks hitherto. He points out that the Tyer apparatus, with or without the Jousselin indicator, with which certain companies have sought to establish the block system, are of delicate construction, and require constant supervision by the railway servants, and the same remark applies in a lesser degree to the Regnault indicators. This they must do by the adoption of perfected apparatus, combining solidity of construction with simplicity of working, and presenting the following conditions :—Immediate and complete solidarity of operation between the electric and the sight signals, so that the latter may automatically interpret the former; a mechanical contrivance by which the signal can only be altered from the next post in the direction taken by the train, and, in the event of the electric apparatus breaking down, the maintenance of stop signals. These conditions appear to be satisfactorily fulfilled by electro-semaphores of MM. Lartigue, Tesse, and Prudhomme, which are in regular operation on certain of the chief sections of heavy traffic on the French lines.—From *Liverpool Journal of Commerce*, Feb. 1, 1882.

CITY NOTES.

Old Broad Street.

THE EASTERN TELEGRAPH COMPANY, LIMITED.

At two o'clock on Thursday afternoon the nineteenth half-yearly ordinary general meeting was held at the Cannon Street Hotel. Mr. John Pender, M.P., Chairman of the Company, presided, and there was a numerous attendance of shareholders. After the reading of the minutes, the report (an abstract of which appeared in our last issue), having been taken as read,

The Chairman gave a brief *résumé* of the principal figures appearing in the report, emphasising the unfortunate fact that during the past half-year the company had to effect a very large amount of cable repairs. The duplexing of our cable apparatus, he mentioned, had cost £15,065. That had been dealt with as capital, but in any further duplexing for shorter distances they were taking the cost from revenue. He then went on to say: Now, gentlemen, before asking you to second the report, it is my duty—my painful duty—to intimate to you that since we met here last we have lost two of our directors. One of them, Mr. Massey, had been connected with this company from its formation. He was a man of large experience, very sound judgment, and during the ten or twelve years in which this company has been established, the benefit of that experience and judgment was most valuable. He has now gone. Of course we regret, and deeply regret, his loss. In the same way, Col. Glover, who, as many of you are aware, was an engineer in the Indian service. When he came over to this country and retired, we availed ourselves of his services, and he continued with this company, as director, up to the day of his death, in December last. We also lost a valuable and experienced director in Col. Glover. We have considered carefully the future of this company. It is a company which requires a great deal of personal and daily attention; and we have thought that, looking to the peculiar character of the work which has to be done, it wonld be right to introduce to the board two younger men. As to the other members of the board, I cannot say we have got to the period of life when we are unfitted in any way for our duties, but we think it is better to introduce some younger blood so that they may acquire a thorough knowledge of the working of an institution like this, and be able to take the more important part which will come upon them in later years. In that way we have introduced to the board Lord Sackville A. Cecil. I may say, without hesitation, that he is one of the eminent electricians of the day, and that therefore we acquire valuable scientific information by his becoming one of the directors (hear, hear). He has also, I may tell you, had considerable experience in cable-laying. He was in the employment of this company for some time; and I think we are fortunate in securing Lord Cecil. The other young member we have introduced is a son of my own. It may be in your recollection that about six months ago, when I had the pleasure of addressing you, I intimated, or it was intimated, that I had lost a son who was in the service. That son, whose intention was that his life should be devoted in a great degree to the interests of telegraphy, and he was introduced into the company to aid and assist to a certain degree Sir James Anderson, as you are aware, died. We have introduced now my younger son, who has had considerable experience of business, and I hope he will be able to devote in the same degree and in the same way that we intended his elder brother to devote, his best services to the interests of this company. I hope the policy we pursue—and we have considered it well in the interests of the shareholders—is a policy that will be adopted by the general body of shareholders. Now, gentlemen, I think it is right also to mention to you, that having regard to repairs, which I have stated have been very heavy indeed, that our cabies are now ten years old. It has been stated some few years ago that the life of a cable was something like ten years. We believe at the present moment, judging from the cable we have taken up from the deep, that our cable generally is pretty well, so far as the core is concerned, as sound as upon the day it was laid down. Although the cost of our repairs is considerable—in those repairs we have executed since the company was formed—we have laid down quite a thousand miles of new cable, so that every repair we make will leave the cable in a more sound and satisfactory state. We are writing off every year this amount which we expend on the new cable, consequently we are placing our property on what we believe to be a position so sound that we may look forward with as much certainty to the ten years before us—a great deal more certainty, in fact, than we looked forward to the first ten years of the existence of this company. There is no question at all now but we have our property in hand; we know that if a break occurs we can repair it. I shall give you an instance of that in a telegram we have just received from the ship *John Pender*, which is now at work on one of our cables in the Bay of Biscay:—"Corunna, 8.35. 1 Feb. Since leaving port grappled for the cable north of the only position where it had yet been hooked. Caught it in latitude 44″5; longitude 9″1, on the 28th, and picked up towards the fault, but after four and a half miles had been recovered, the weather rendered it necessary to cut and buoy. Returned to the buoy on the 30th and picked it up, but our moorings parted. The latitude, however, of the east and west line then being rendered more precise, several sweeps were made further east, and last night the cable was hooked nearly half a mile west of the fault in latitude 44″10, longitude 8″50. The fault was a chafe, 360 fathoms. The depths are very irregular, and bottom rough in places. Now blowing easterly gale with high sea." That was the Lisbon cable. The result of that is that they have the end of the cable, and I hope within forty-eight hours, if the weather is at all settled, that this cable will be repaired. There was a time when a cable breaking in the Bay of Biscay it was almost considered impossible to repair it. Therefore, gentlemen, I think I am justified in saying that we have the property so well in hand now that when a fault occurs we can pick it up, put in a new piece of cable, and leave it in a thoroughly satisfactory condition. So much for repairs. I should also have mentioned that during that time we have laid down sixty-one nautical miles of shore-end cables. Shore-end cables are a much more expensive style of cable, but they get considerably chafed either by being acted on more by the waves near the shore, therefore we have to replace them more frequently. They were replaced entirely out of revenue. Since I last had the pleasure of talking to you we have endeavoured, and I have always been endeavouring, to maintain friendly relations with governments bordering on the Mediterraneau and the Levant. We have just concluded an arrangement with the Greek Government to connect, I believe, all the Greek islands, with the main system. These of course will be a feeder to a considerable extent of our existiug lines.

We have made arrangements with France. I think I mentioned at one of these meetings some eighteen months ago that we had some difficulty in our Algerian traffic with France—that she had not treated us as liberally as we thought she ought to have done. I made the remark that the minister of the day was new to telegraphs, and that possibly when he had remained longer in office and gained greater experience, we probably would come to understand one another better. It has always been our policy to keep up friendly relations with foreign governments as far as we possibly could. The Minister of Telegraphs, M. Cochéry, has certainly borne out what I said of him at the time. He has shown an amount of intelligence, an amount of energy, a desire to give cheap telegraphy to the country, perhaps greater than any minister that we have come in contact with. He has now realised the position of the submarine companies, and he is perfectly satisfied that they are of an international character, and that they ought to be supported as such. We have, therfore, entered into such friendly relations with France in regard to the Algerian and other traffic, that I may say now what I could not have said a few months ago, as to the threatened opposition between Egypt and France, and the statement that then was receiving a certain amount of encouragement from the French Government. The speaker here was scarcely audible, but he was understood to remark: I believe that is legerdemain. I believe it is the interests of the public as well as of our shareholders that we should command the Mediterannean and the Levant. Our system is broadcast over these waters, and I think it would be almost impossible for any competing company to come in with any amount of success against us. Again, gentlemen, with regard to Turkey, our relations are of a most satisfactory kind. In regard to Italy, I should have said equally so. We are now negotiating with Italy for certain extensions, and for the working of a certain submarine system. Therefore, taking it altogether, our position in the Mediterannean and the Levant is of a most satisfactory kind (hear). There is one point that I would refer to, though with a certain amount of delicacy. It will come before you at a special meeting when we are prepared to speak more definitely on the subject. As I have told you, it is of the greatest possible importance that we should hold our own in the Mediterannean and the Levant. Austria, like France and other nations, is very anxious to have communication of its own with Egypt. They have twice endeavoured to bring that about. Twice we have been successful in keeping the thing in our own hands. They have now granted a concession. That concession has been obtained, I am glad to say, by friendly parties. The cable has been made by the Telegraph Construction Company, and we are in negotiation now to secure that cable as part of our system. The cable is from Trieste to Corfu, where it joins our system; and I believe it will now be satisfactory to the Austrian Government that we should have the working of this system. I may tell you it requires some little delicacy in dealing with these governments, because sometimes there is considerable jealousy existing. There is on the part of some of these governments also, a belief that competition is a very good thing, and therefore in securing this concession we had to do it in a way that would so far prevent anything like jealousy, or a statement that we were endeavouring to become great monopolists. On that point, gentlemen, we are no doubt monopolists in these waters, but if you will take the history of these submarine cable companies, and see the reductions that we have made in tariffs, and the benefit we have conferred on those who are using telegraphs and compare them with what, I might say, our own Government has done during the ten years they have had charge of the land lines, and you will find the change is something marvellous. Still, gentlemen, with all these reductions and benefits conferred by our submarine telegraph system upon the commercial, and I may say, political and social, world, we have still been able to give you, though not a large, still to maintain a steady, respectable dividend, and I hope a growing one (hear, hear). With regard to this Austrian business, I think I have said sufficient for the present time. Before we come to any conclusion, at all events before we ask for any assistance in any way, we shall call a special meeting and lay the whole question before it. It may necessitate also a change in our debentures. We have next year some £232,000 six per cent. debentures maturing, and it may be necessary then to make some re-issue of debentures, I hope at a very considerably lower rate. I hope the money market will be in a better position then than it has been in for the last two or three years, to enable us to get the money at a lower rate. In that reconstruction or re-issue we shall deal with the question of the Austrian cable if it is decided it should be the property of this company. I would just make one remark, and that it this: there has been a deal of correspondence lately in regard to interruptions to the repairing of cables on our own shores. As you have read in the newspapers, a good deal of correspondence has taken place. Submarine cable companies now possess something like £27,000,000 of property. They are taking up the position politically, commercially, and socially, that they are indispensable, and I am glad to say that the Government—our own Government—has taken up this question, endeavouring to make it an international question. Germany has seconded the efforts, France is approving, Spain is approving, therefore I am hopeful that our property will have a certain amount of security by some understood international law which will at all events enable us to repair our cables without obstruction, and in the event of cables being dealt with maliciously, we should have some mode of being recouped for any damage. I think I have pretty well exhausted my remarks, and now, before putting this resolution, if any gentleman has any suggestion to make, I shall be glad to listen to him and give him the best answer in my power. The resolution is that the printed report and accounts of the directors to September 30th, 1881, now submitted, be hereby received and adopted.

The Marquis of Tweeddale, vice-chairman, seconded the motion.

Mr. Marks, shareholder, remarked that the company had been in existence three or four years, and in regard to facts they had nothing at all before them except that they had received a dividend. There was no account taken if they were increasing or decreasing—in fact, no details whatever were given.

Mr. Newton said that in one matter concerning the company he most decidely objected to the addition made to the board within the last few weeks. He thought they required gentlemen of mercantile experience rather than the two young gentlemen proposed for the directorate. Considering the large business of this company, and the various countries through which our cables pass, they should not only have large commercial experience, but they should have had experince of many years in the work. There are young men who have mixed in questions regarding telegraph companies, but there is another side to the question. They may have sound capabilities of this character, but we want something more than that on this board, and more of the commercial element than the young gentlemen could possess. I am sorry it falls upon me to make these remarks, but it must be specially understood that no nepotism or anything approaching it can be permitted in great undertakings like this (hear, hear). I was glad to hear the chairman tell us that it required personal daily attention at the office of this company. Now, sir, I have in my experience made remarks at these meetings with reference to the conduct of the company, which I think justify me in saying that daily and personal attendance cannot be given by gentlemen who occupy seats at so many boards. We have too many pluralist directors at this board, and I think that there should have been some little delay before Mr. Pender had appointed his son. As you all know I have frequently opposed Mr. Pender, but I should be very sorry indeed to see his name removed from the telegraph companies, and I am very glad indeed to state that I do not object to his son because he has not rendered great services to telegraphy; but I cannot shut my eyes to the fact that this company can pay very well for his services, and he has no need to go to any other company to work in any character not connected with telegraphy. You will find Sir James Anderson connected with a good many companies. I wish to say, Mr. Chairman, that there has not been in the management of this company that perfect success which would justify us in accepting others upon the directorate. I have taken the figures in the history of this company, and as many of you are aware we began with the capital of £3,300,000. We paid dividends of 7 per cent. We increased our capital, and now it is $5\frac{1}{2}$ millions, and our dividends have gone down 5 per cent. ("Six") announced at the close, of the half-year. But, however, we want, as the chairman has said, a capable man who has long experience and sound judgment, like Mr. Massey, and, therefore, when these additions to our capital have taken place such men are invaluable to us. I have not very much to say, except this, that telegraphy pays most handsomely to the gentlemen who have charge of it. It pays something like £25,000 per year to its directors; and if you will search the list of telegraph companies you will find that these gentlmen are all connected with one another, that there has been a great deal of animosity to these companies which would not have existed if such names had not occurred so often in their directorate. I have noticed that these two young gentlemen are appointed as directors under the powers given to the board, not by the shareholders, and I do not think, as I have before addressed you on this subject, that the directors should barricade themselves by such improper articles of association. They have the shareholders absolutely powerless, and you could no more refuse to re-elect any of the gentlemen that are retiring now than you could fly. They are re-elected absolutely by whatever this meeting may do. Even if there were no proxies they are elected. The directors, even beyond these articles of association, have formidable powers of proxies. I do not think that the money expended in proxies is absolutely wasted, for the company might be scattered to the winds, and yet the directors could save nearly every farthing of that money. There is no necessity to spend large sums on these proxies. It would never do to let a great property be at the mercy of chance meetings. I might, and you might, get a clique together and pass resolutions that would utterly destroy the company, and that would bar all progress. But in a business like this, I think that sending proxies to every one of the 8,000 shareholders is an unnecessary waste. Hence, I do trust that you will do away with proxies. I trust you will accept my remarks as made without any personal feeling. I have not the slightest feeling against any one of these gentlemen, but one word of advice I should like to offer to Mr. Pender—that he should get his name off those rubbishing gold mines, and other such companies. Let him devote himself to telegraphy, which he will find will prove ten times more valuable than when he is associated with so many other companies.

Mr. Abbot said: Mr. Chairman and gentlemen, I think the remarks of the last speaker were somewhat contradictory. In the first place he objects to proxies; and in the next place he urges their importance. In the first place he objects to Mr. Pender, junior, and then he urges his election on condition that he withdraws himself from some other undertakings. I think these remarks a little hard, because I happen to know something of Mr. Pender in connection with some of those Indian companies which are regarded as rubbish.

Mr. Griffiths said that he was glad that the word *extraordinary* had been deleted from the report. Expenses which occur year after year can scarcely be called extraordinary.

Mr. Hurst said that he would not attempt to criticise at length the interesting information given by the chairman as supplementary to the report. There was, however, one statement which was somewhat pleasing—the information that they were interested in negotiations with Austria showed at least that they were making progress, and availing themselves of any opportunity that offers to extend the future of their company. I would like to suggest whether it would not be acting for the good of the company to raise the increased capital by shares offered to the shareholders at par. It would be a reward to those long connected with the company. It is the principle adopted in our large companies, *e.g.*, in the new stock of the London and North Western, some was offered at par to the shareholders, though in the market it was as a large premium. I make this suggestion because it seems proper that the directors should consider the point. One other point upon the directorate. There is no doubt we are goverened by a very able body of nen—men of statesmen-like character, and also of commercial experience. With such a man as Sir James Anderson as our electrician, there is not much risk incurred by introducing gentlemen whose fault is said to be their youth.

Major Leslie, who said that his family were amongst the largest holders in the company, remarked that if they were to follow out their lines of development in the great European complications now arising, they would be committing a great mistake. He thought that the gentleman who spoke against the election of Mr. Pender, junior, was eloquent in a mistaken cause—he had great readiness of speech but false arguments and false facts. He was ingeniously mistaken (laughter). They ought to carry the resolution to elect these gentlemen unanimously.

Mr. Roberts said that with regard to the list of investments they of course required a reserve fund, but he did not consider it necessary that such a large sum as £60,000 a year should be so invested. He thought they ought next year, when the 6 per cent. debentures fell due, to pay them off with it (laughter). He did not see either why some of that £60,000 might not be added to the dividends.

The Chairman said: I shall endeavour, as far as I possibly can, to answer the remarks made by the various speakers. Mr. Marks has referred to the South African Company, as he has referred on previous occasions to the same, I stated then, and I state now, that no act of your directors has been more beneficial to the interests of the Eastern Telegraph Company than that in which the South African Company has been concerned. If we had gone to the public at that time we would never have got our money; but we arranged with the Telegraph Construction Company, we found the money, and we laid the cable. A very large proportion of that cable now belongs to our company. We are getting 10 per cent. return upon our capital invested, we are putting past a reserve fund; and, as I told you on the last occasion, though I may not live to see it, many of those in this room will live to see the South African Company belonging to the Eastern Telegraph Company without extending in their books at one shilling of cost. This is some evidence of what your directors have done in the past. I would say further, in answer to Mr. Newton, that he has I must say, considering his peculiar style, that he has dealt very fairly, given a very fair criticism upon what the board has done, and I accept it in the spirit in which he has given it. But I ask you, gentlemen, to look at your board, what they have done. Look at the last ten years' experience. What telegraph company has maintained its position as we have done? We have very largely increased the power of telegraphy through the duplexing of our cables; in fact, if we had not kept ahead of other cable companies we could not have been paying the dividend we are paying to-day. At the present moment when we look at the fluctuations going on in almost every other company how steady are we! You are getting your five per cent., and I hope when I meet you again to be able to say that you are entitled to a bonus. I am proud to say that the present half year is the best for us, so far as returns are concerned, that we have had since we started. I hope, therefore, there will be some result to the shareholders out of this improved state of things. I do not ask you to judge us by what we are going to do in the future, but by what we have done in the past. In reference to the noblemen now on the board I am quite sure that I could convince Mr. Newton in five minutes' conversation that what the board has done in obtaining them has been a wise and judicious thing. I could show him that not only in regard to the nobleman brought in, but also in regard to the noblemen who have had to do with the company since it was formed, that the position of these noblemen, backed as it is by sound sense, has been of enormous benefit to such institutions as this. As to my son, I am not going to say one word about him, only to ask you to look at what he has done, young as he is, and I think the position we have placed him in is completely justified. I was myself, when five or six years younger than he is now, conducting a business almost as large as the business I am presiding over at the present moment. I wish a young man to begin early, to gain sense of the responsibilities and duties of life, and I believe that he will faithfully perform these duties to the satisfaction of the shareholders. Now, in regard to proxies, I ask Mr. Newton, have we ever used *his* proxies? We have rarely, thanks to the good sense of the large majority of our shareholders, had to come to a division on any question. They have accepted the policy of the directorate with confidence. I thank you, on behalf of my colleagues, for that confidence. I think we have not in any way betrayed it. There are larger operations coming forward, on which we require to consult you, but depend upon it, so long as the present directorate are in power we shall be at the head of the telegraph systems of the world. I am not boasting, I am speaking what I believe to be a great fact. Reference has been made—I shall say it in your presence—to the gold mines of India. You may think I am going out of my way in making this allusion, but it has been introduced to the meeting. I am of the same opinion as I have always been as to gold mines, that if it had not been for the discovery of gold in California, and afterwards in Australia, we would have been poor at this day—it would have been impossible that the great prosperity of the last 40 or 50 years could have existed. There would not have been the means to carry on our commerce. I believe that what was in India once may come to India again. Modern machinery may enable us to find gold there, and if found, what an enormous benefit it will be to this Eastern Telegraph Company. Take the great success which has attended our South African Company. How has it made its progress? War has had something to do with it, but the diamond fields more by a great deal. We have not come to the end, and we are not coming to the end, of finding gold whether in India or elsewhere. With many of the schemes floated I do not agree, but I say that there is a prospect of finding gold in India, and that will enormously benefit that country. One general remark on a large reserve fund. It is almost a necessity in companies like this. We are adding to the fund, and as I said we hope at the same time to be able to add something to the dividends. Mr. Griffiths spoke of raising a fresh capital. We have not come to that point yet, but when our scheme of finance, six months hence, if not sooner, is laid before you, we shall tell you exactly how we shall deal with that matter—but you may rest assured that the shareholders of this company will always be the first consideration of the directors in the allotment of any debentures or shares offered.

With these remarks I have pretty well met all the questions raised and I thank you for listening so patiently to my reply.

Mr. Newton said he was very much obliged to the chairman for his explanation, and he thought the shareholders should be obliged to him for having elicited these remarks upon the future of Indian gold mining.

The Chairman said he was always glad to hear remarks from Mr. Newton, or any other shareholder, which elicited opinions from the board which would not be otherwise expressed. He hoped the little discussion to-day had been, at all events, a friendly one, and he thanked Mr. Newton for the spirit in which he criticised the report.

The report, moved by the Chairman, and seconded by the Marquis of Tweeddale, was then put to the meeting, and carried unanimously.

The Chairman said that the next resolution was that the most Honourable the Marquis of Tweeddale be and hereby is re-elected a director of this company. The company had received the greatest possible support from Lord Tweeddale since he became a director of this company. His knowledge of India from his long experience there, and his general knowledge of business, were such as to be exceeded by no man, and he put this resolution in the perfect belief that they would maintain on the board a man most valuable to them.

The resolution was carried unanimously.

The Chairman: The next vote is that Mr. George Gorden Nicol be hereby re-elected. From the first of my commercial days I have been associated with Mr. Nicol, and I have found that he is what I call one of the handiest of directors. We have in him a most efficient and excellent working member of the board.

The resolution was carried unanimously, as was also the next, that Mr. Henry Dever, and Messrs. Quilter, Ball, and Co., be re-elected as auditors.

On the motion of Mr. Abbott, a vote of thanks to the chairman and directors was carried, and the proceedings terminated.

LATEST QUOTATIONS.

Authorised Issue.	Share.	Name.	Paid.	Closing Quotations. Feb. 1.	Busines Done.
		ELECTRIC LIGHT.			
40,000	10	Anglo-American Brush Co.	4	8¾- 9¼	9¼.⅛.¼ xd
		Do. Do.	10	21 - 22	22 xd.
150,000	1	Electric Light and Power Generator Co...........	1	1 - 1¼	1⅛
		TELEGRAPHS.			
2,116,400*l*.	Stk.	Anglo-American, Limited	100	49½- 50½	50
2,441,800*l*.	Stk.	Do. Preferred } Def'd. receiving no div. until	100	77½- 78½	77½.¾
2,441,800*l*.	Stk.	Do. Deferred } 6 p. c. has been paid to Pref.	100	21½- 22½	21¾.2
130,000	10	Brazilian Submarine, Limited	10	10⅞- 11⅛	11⅛
16,000	10	Cuba, Limited	10	8½- 9	xd.
6,000	10	Do. 10 per cent. Preference	10	15½- 16½	xd.
13,000	10	Direct Spanish, Limited	9	5 - 5½	
6,000	10	Do. 10 per cent. Preference............	10	14½ 15½	
65,000	20	Direct United States Cable, Limited, 1877	20	10 - 10¼	xd.
100,000*l*.	100	Do. 6 per cent. Debenture, repayable 1884	100	100 -103	
380,000	10	Eastern, Limited........................	10	9⅞- 10⅛	9 15/16-10
70,000	10	Do. 6 per cent. Preference	10	12½- 12¾	
232,000*l*.	100	Do. 6 do. Debentures, repayable Oct. 1883	100	101 -104	
200,000*l*.	100	Do. 5 do. do. Aug. 1887	100	98 -101	xd.
200,000*l*.	100	Do. 5 do. do. Aug. 1899	100	102 -107	xd.
199,750	10	Eastern Extension, Australasia & China, Limited	10	10⅝- 10⅞	10¾ xd.
320,000	100	Do. 6 p. c. Debentures, repayable Feb. 1891....	100	105 -109	
500,000	100	Do. 5 p. c. (Australian Gov. Subsidy) Deb. 1900	100	101 -104	
140,000	100	Do. do. registered, repayable 1900	100	101 -104	
100,000*l*.	100	Do. 5 per cent. Debenture, 1890................	100	102 -105	
254,300*l*.	100	Eastern and South African Limited 5 per cent. Mort. Deb. Registered redeemable 1 Jan. 1900	100	100 -103	
345,700*l*.	100	Do. do. do. To Bearer ..	100	100 -103	
22,050	10	German Union Telegraph and Trust, Limited	10	9¼- 9¾	xd.
163,390	10	Globe Telegraph and Trust, Limited..............	10	6 - 6½	
163,209	10	Do. 6 per cent. Preference..............	10	12¼- 12½	12⅜
125,000	10	Great Northern	10	11¼- 11¾	11½
100,000*l*.	100	Do. 5 per cent. Debentures	100	101 -104	
31,200	10	India-Rubber, Gutta-Percha and Telegraph Works	10	23½- 24½	25⅜.4⅛
100,000	100	Do. 6 per cent. Debentures, 1886	100	104 -108	
17,000	25	Indo-European, Limited........................	25	28 - 29	
38,148	10	London Platino-Brazilian, Limited	10	4¾- 5¼	
12,000	10	Mediterranean Extension, Limited	10	2¼- 2¾	
8,200	10	Do. 8 per cent. Preference............	10	9¼- 9¾	
9,000	8	Reuter's, Limited	8	12¼- 12¾	
280,000	Stk.	Submarine	100	270 -280	
58,225	1	Do. Scrip	1	2¼- 2½	
4,200	Cert.	Submarine Cables Trust	100	97 -100	
37,350	12	Telegraph Construction and Maintenance	12	28½- 29½	29¼
150,000	100	Do. 6 per cent, Bonds, 1884 ..	100	101 -105	
186,750	5	Do. 2nd Bonus Trust Cert. ..	5	1⅜- 1⅝	
30,000	10	West Coast of America, Limited	10	4½- 5	
150,000	100	Do. 8 per cent. Debentures	..	..	
69,910	20	Western and Brazilian, Limited	20	8½- 8⅝	8½.¼.⅜.½
200,000*l*.	100	Do. 6 per cent. Debentures "A" 1910	100	105 -110	xd.
2,500	100	Do. 6 p.c. Mort. Deb. series B of '80, red. Feb., 1910	100	97 -100	xd.
1,500	$1,000	Western Union of U. S. 7 p.c.1 Mort.(Building) Bds.	$1,000	123 -128	
1 030,000*l*.	100	Do. 6 per cent. Sterling Bonds	100	104 -107	
88,321	10	West India and Panama, Limited	10	1⅝- 1⅞	
34,563	10	Do. 6 per cent. 1st Preference	10	7¾- 8¼	7⅞
4,669	10	Do. 6 do. 2nd do.	10	6 - 6½	
		TELEPHONES.			
154,165	1	Con. Telephone & Maintenance, Ld. Nos. 1 to 154,165	1	1¼- 1½	1 7/16
200,000	1	Oriental Telephone Co.	½	⅜- ⅝	9/16
100,000	5	United Telephone Co.		12½-13	13¼.⅛.12⅝.½

EDWARD PATERSON,

TELEGRAPH & ELECTRICAL ENGINEER,

St. Paul's Works—76, LITTLE BRITAIN, LONDON, E.C.,

AND

European Telegraph Works—POWNALL ROAD, DALSTON.

(PARIS AGENTS—E. & S. DELEVINGNE, 8, RUE MARTEL, PARIS.)

Prize Medal, International Electrical Exhibition, Paris, 1881.

MANUFACTURER OF

Telephones, Transmitters, Switchboards,

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